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ORIGINAL ARTICLES.

I.—Notes on the Ponza Islands.

By H. J. Johnston-Lavis, M.D., B.-ès-Sc., F.G.S., etc.

In 1884 I spent about a fortnight investigating the geological structure of these islands, and intended to have to a certain extent completed my studies at that time. Cholera was then raging at Campobasso on the mainland, and the islanders, not comprehending the curious habits of a travelling geologist, supposed I had been sent by the Government to spread the "Cholera Powder," and requested me to leave the islands by the next steamer. This I naturally for safety's sake did, and opportunities for returning to the locality not having again presented themselves to me, and lest I should forget some of the interesting observations I then made, I think it desirable without further delay to record what I then saw. This is all the more important since my own observations differ considerably from those of the few other observers of the geology of these islands, or include facts which they do not mention in their writings.

Santo Stefano is of exceeding interest from the very clear sections which its steep cliffs near the Cemetery present. The following are the details of the section:

		M.
(*a*)	Breccia with vegetable soil and modern pottery together with made earth	1·50
(*b*)	Compact brown tufa with weathered pumice, probably an old vegetable soil	1·30
(*c*)	Ditto, but not compact and containing concretions, especially at bottom, where it is a little more sandy	1·10
(*d*)	Two or three pumice-beds with intermediate ash-beds irregularly interbedded and concretionary along fissures	2·20
(*e*)	Dust-bed somewhat irregularly reposing on pumice-bed	0·50
(*f*)	Pumice-bed, the lower part is about m. 0·10	0·65
(*g*)	Black sand or lapillo bed finely stratified; coarser as it graduates down to next	0·55
(*h*)	Pumice and pumiceous scoria	0·55
(*i*)	Earth buff fine-bedded pumice, with lapilli ?	0·30
(*j*)	Loose pumice and lapilli	0·90
(*k*)	Very fine buff dust, finely stratified, containing bits of pumice and is also concretionary	1·00
(*l*)	Slightly concretionary dust-bed, yellow at top, purple-brown at bottom and graduating thence into fine stratified dust; the yellow bed is an old vegetable soil with concretions that have formed around roots and fills in the eroded surface of the purple-brown bed, showing weathering had occurred at the surface	1·78
(*m*)	Fine lapilli with concretionary bands	0·30
(*n*)	Pumice-bed	0·30
(*o*)	Fine black scoria	0·14
(*p*)	Fine greenish-grey dust with band of lapilli at bottom	0·28

(*q*)	Irregular finely (occasionally) brecciated yellow earth; probably an old concretionary soil	1·00
(*r*)	Ochre-stained pumice; the upper ten centimetres are less stained, have larger fragments with pyroxene crystals and ejected blocks	0·90
(*s*)	Brown earth with concretions, finer at the top and passing down into	2·60
(*t*)	Same as last, lighter in colour with intermixed pumice which at some spots forms a band	1·20
(*u*)	White dust not concretionary	0·80
(*v*)	Brown pumice	0·25
(*w*)	Pumice bed with lapilli and few ejected blocks	2·00
(*x*)	White pumiceous dust-bed	0·15
(*y*)	Pumice-bed with light buff concretionary dust-bed in middle	1·85
(*z*)	White pumiceous dust-bed	? 1·50
(*zz*)	Trachyte forming main mass of the island.	

The main body of the island is composed of a mass of trachyte which seems to have oozed forth as a compound dome. The flow arrangement resulting from this is well shown as the rock exhibits a distinct pipernoid structure. Doelter has fully described[1] this rock both from a chemical and microscopical point of view, but says little of the overlying strata of pumices, lapilli, dust and earth. These all point to a considerable number of explosive eruptions in the vicinity, as there are no signs of a crater of explosion visible on the island itself. That during the deposition of these tuffs the island was above the water is proved by the numerous vegetable soils, roots preserved by concretions, surface erosion, etc. That the explosive eruptions extended down to the metamorphosed limestones beneath the volcanic platform from which occurred the explosive eruptions is proved by the few ejected blocks met with in the tuffs. The ejected blocks also include lava fragments like the underlying trachyte and the dolerite of Ventotene, but most interesting is a leucitophyre, no massive remains of which have been discovered in the islands. The latter, nevertheless, is another addition to the widely-distributed localities of Italy in which leucite forms an important rock constituent. Loose blocks have been found in tuff at Procida, but so far leucite rocks have not been recognized in Ischia or the Ponza group of islands.

Ventotene is also exceedingly interesting, as the continuous cliff section stretching round it clearly reveals its structure. The following is a compound section of the island, beginning from the most superficial deposits:—

		M.
(1)	Vegetable soil, rich in snail shells	—
(2)	Blown sand of powdered sea shells with many snails, chiefly *H. Cantiana*. It is very full of concretions	4·00
(3)	Brown earth with concretions especially at bottom, often with bands of blown sand	7·00
(4)	Compact yellow tuff, used for carving into portable furnaces, passes down to	4 to 7·00
(5)	Compact grey dust and pumiceous scoria with bands of loose pumiceous scoria in the middle	0·90
(6)	Scoriaceous pumice with pieces of lignite	0·05
(7)	Lapilli consolidated in patches	0·20

[1] See Doelter, "Carta geologica delle Isole Ponza Palmarola e Zannone," Roma, 1876.—Memorie per servire alla descrizione della Carta Geologica d'Italia, vol. iii. Parte 1ª, 1876.

(8)	Brownish red dust bed	0·25
(9)	Pumice bed decomposed at bottom	1·00
(10)	Red ash bed	0·40
(11)	Pumice bed. The pumice often compact from calcareous infiltration ...	1·10
(12)	Stratified pumiceous dust, red at top	0·65
(13)	Slightly decomposed pumice bed	0·90
(14)	Finely stratified black dust graduating down into	0·60
(15)	Pumice and lapillo	1·80
(16)	Same, but with many fine ash bands and more compact	0·60
(17)	Grey pumice and lapillo	1·60
(18)	Fine and coarse buff-grey stratified dust, with pumice at bottom. It is compact, pisolitic, and concretionary	4·80
(19)	Brownish earth, with scoria and pumice, a very irregular bed	1·20
(20)	Finely stratified greenish dust	1·60
(21)	Pumice and lapilli, mostly fine	1·00
(22)	Finely stratified buff dust	1·50
(23)	Pumice and lapillo bed, fine, but more so above	1·00
(24)	Stratified buff-grey dust, coarser in stripes with bits of pumice	1·60
(25)	Pumice-bed, with thin ash bands and finer in upper half	1·13
(26)	Concretionary bed of ash	1·10
(27)	Lava and scoria intercalated in tufas at south-east angle of the island. Maximum height	20·00
(28)	Red earth with pumice and small lava fragments...	0·55
(29)	Band of decomposed pumice, finely bedded	0·15
(30)	Fine red earth with concretions	2·00
(31)	Pumice-bed with ash bands...	1·80
(32)	Breccia, ash dust, with pumice, banded, probably part of last	0·35
(33)	Pumice-bed passing up into red dust at top	0·95
(34)	Red earthy breccia, stratified with scoria fragments	2·30
(35)	Doleritic lavas.	
(36)	Stratified lapilli and dust, red at top, possibly marine	3·00
(37)	Brown and yellow scoria ... ... over Sea-level	5·00

It would be out of place here to go into all the details of each separate deposit. I shall therefore simply point out the general conclusions to which I have been led from their examination.

The earliest deposits visible are brown, yellow, and red scoria, lapilli, and dust, presenting characters which indicate that they were basic essential ejectamenta from a cone in a state of activity, not unlike some of the more active phases of Vesuvius. This cone seems to have been situated somewhere west of the south end or perhaps west-north-west of the island, as this point shows the oldest deposits at the highest level, and represents parts of the slope of that cone. Also the valleys, if such they may be called, radiate from such a theoretical point.

These deposits were followed by a great and continuous outflow of doleritic lavas forming the base of the southern prominence of the island. They have been studied microscopically and chemically by Doelter. A field study shows them to have been poured out almost continually without the intermission of extrusive action, as practically no fragmentary materials separate the different streams. In many places they show beautifully corded surfaces which would indicate their poverty in dissolved water at the time of their issue, the phenomena of which must have much resembled the eruption of 1858 at Vesuvius. The existence of the island depends upon the presence of these lavas extending below the surface of the sea, and

resisting the powerful scirocco breakers. It seems to me that these lavas issued by some lateral fissure in the old cone, and piled themselves up at this point, just as many of the eruptions of lava of Vesuvius may have been observed to do during the last ten years, just resembling in fact the guttering of a candle.

The volcano then seems to have remained inactive for some time during the deposition of (34), but during that inactivity at the surface, the magma immediately below was gradually assimilating water, or in other words rising in elastic tension. This eventually overcame the overlying obstructions, and a series of explosive eruptions followed, interrupted by periods of repose (34–28). Whether these explosions drilled out a great crater or craters in the old cone, or were at some distance, is not possible of determination; but it is not improbable that the ruin of the dolerite cone had been in part brought about.

Bed (28) seems to show that repose did not follow the last of the explosions, but that the eruptive action continued, and as a consequence dwindled into a state of chronic activity, probably repairing in part or wholly, the old cone, during which time fine ash and lapilli, now red clay and lapilli, covered the surface (26). At any rate, a dyke forced its way to the surface, and gave rise to a small scoria cone, from which one or more lava streams poured forth. This scoria cone is seen on both sides of the island, and I give in Figs. 1 and 2 its relative position to the pumice-beds and the old lavas. Fig. 3 is a diagram showing how it is probably cut by the present cliff.

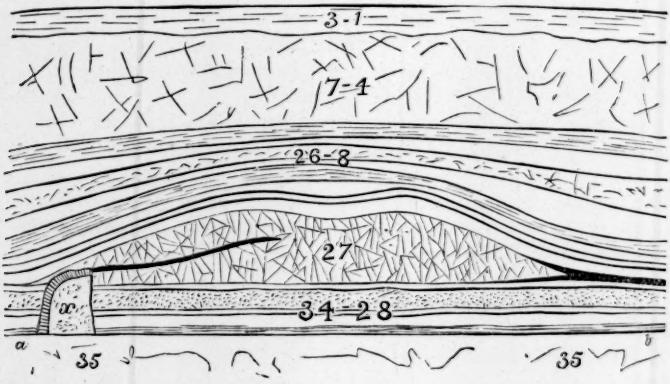


Fig. 1.—Section of a Volcanic Cone and Tuffs N.E. of the Piano degli Olive at Island of Ventotene.

35. Submarine reefs of amygdaloid lava.
34–28. Pumice and other explosive eruptive products.
27. Doleritic scoria and lavas forming a small volcanic cone.
x. Dyke which probably supplied same.
26–8. Upper pumice and other explosive eruptive products.
7–4. Compact tufa.
3–1. Vegetable soil, blown sand, etc.
a, *b*. Sea-level.

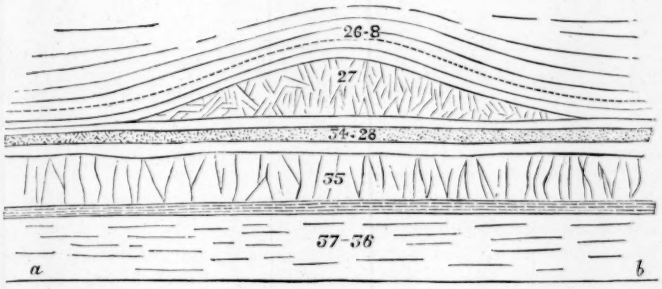


FIG. 2.—SECTION OF LAVAS, TUFFS, PUMICES, AND A SMALL VOLCANIC CONE BETWEEN "IL TELEGRAFO" AND THE "TERRA ABBANDONTA," VENTOTENE.

a, *b*. Sea-level.
37–36. Scoria, dust, etc.
35. Doleritic lavas (lower).
34–28. Lower pumices and other explosive eruptive products.
27. Doleritic (upper) scoria forming section of toe of small volcanic cone.
26–8. Upper pumices and other explosive eruptive products.

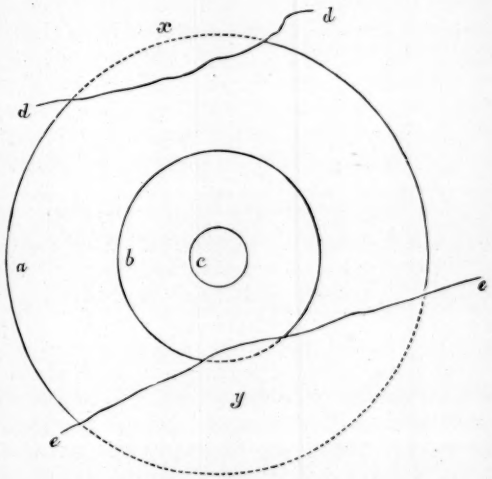


FIG. 3.—DIAGRAM OF THE SECTIONS OF A VOLCANIC CONE SEEN IN CLIFFS OF VENTOTENE.

a. Toe; *b*. rim and *c*. vent; *x*. and *y*. parts removed; *d*, *d*. North-west and *e*, *e*. S.E. coast-lines.

With (25) recommenced a series of explosive eruptions (25–8), which culminated in a gigantic one represented by the different pumices, scoriaceous pumices and tuffs (7–4). Of course it is impossible to say at what point all these pumice and dust deposits were erupted, and it is not improbable that they were derived from different eruptive centres. The last seems to have been very near

at hand, for at Ventotene it attains a thickness of 10 m. or more and is distinctly if at all represented at Santo Stefano. It is probably by this eruption that the greatest destruction was wrought in the old cone, and that the crater apex extended down into the subjacent metamorphosed rocks. In fact, the explosive products at Santo Stefano are generally smaller and finer than at Ventotene, and seem therefore to have been derived from a centre or centres nearer the latter island.[1] A grave error has crept into Prof. Judd's "Volcanoes," where the lavas are represented as a stratum covering the tuffs of the island, instead of the reverse.

These different pumice deposits are chiefly derived from a basic magma, probably having the same composition as the doleritic lavas of the island. Of far greater interest from a physical point of view is the regular sequence of the different ejectamenta in each explosive eruption, which I first pointed out at Vesuvius, and have confirmed in a very large number of volcanic deposits. In each we find comparatively vitreous pumice at the bottom followed by micro-crystalline pumice or scoriaceous pumice, and finally a dust bed. Whilst the divisions show a rapid increase of extratelluric "formed material" as the eruption progressed, the intratelluric minerals have not increased in number, the whole confirming the conclusion that the hydration of the igneous magma was limited to near the earth's surface.[2]

The blown sand already mentioned consisted of a mixture of augite and other mineral grains with shell fragments and foraminifera. It must have been carried up on the slopes of the island by the wind from an old beach probably formed on the extensions of the last great tuff deposit, but now cut away. By percolation of carbonated waters, nearly all the subjacent rocks have been permeated and often cemented together or filled by calcareous concretions. We find the lavas near the sea-level also become amygdaloidal by the presence of tufts of aragonite crystals in the vesicles.

Very interesting is the presence of irregular compact white amorphous concretions, often attaining a weight of 50 or more grammes, and composed of almost pure magnesite. These are included in and between the masses of scoria of the buried cone of eruption. They are probably due to emanations at and subsequent to the eruption. I have noticed very similar ones in a vesicular doleritic lava of the east coast of Vulcano.

The next point to which I wish to draw the attention of geologists is the remarkable evidence of elevation to be seen in the island of Palmarola, which seems to have escaped the notice of recent visitors. In the middle of the island is a depression somewhat in the form of an amphitheatre, open to the west and shelving down to the small

[1] The quantity of these explosive ejectamenta is such that the scoria cone was entirely buried in them, so that its existence is only known by the exposures in the cliff sections. See Figs. 1 and 2.

[2] Q. J. G. S. 1884 and Proc. R. Dublin Soc. vol. v. July, 1886; Proc. Geol. Assoc. vol. ix.

beach. On the east coast this depression is, so far as my memory goes, at least seven metres high, and lies to the side of the Forcina dyke. It seems dependent upon the clay through which the dyke was injected being easily subject to erosion. This deposit looks much like a Pliocene clay, but my hurried and forced departure from the island prevented me from more fully investigating it, as also the actual height above sea-level of the lowest edge of the breach. Dolomieu, who visited the Ponza Islands in March of 1786,[1] says: "Cette Ile (Palmarole) est divisée en deux parties presque égales par un canal étroit qui la traverse vers la moitié de sa longueur, et dans lequel on passe en barque." Besides this clear statement he gives a map of the island showing the canal in the position of the amphitheatre depression, and the lowest breach in the east coast-line. At present the lowest part of this depression I noticed strewed with boulders covered with *Serpulæ* and other marine growths, looking quite fresh, but above sea-level. We have evidence here of a very striking kind, of a very considerable elevation during a period of little less than a century—as important I believe as any recorded. I regret that I did not make more detailed observations, but the unfortunate circumstances which caused my departure, together with the fact that it was only after my return that on looking at Dolomieu's map, the fact struck me. I hope, however, to carefully investigate the matter next summer, so as to make public such an important piece of geodynamical evidence. Of course we have other prehistoric evidences of elevation in abundance in these islands, and one in particular that struck me, namely, the occurrence of beds of well-rounded pebbles half-way up the cliff of Monte della Guardia at Ponza. But the elevation of an island from 5 to 10 metres or more in a century would be of inestimable value as a cardinal fact in geology, if fully confirmed.

II.—Terrestrial Magnetism as Modified by the Structure of the Earth's Crust, and Proposals Concerning a Magnetic Survey of the Globe.

By Dr. Edmund Naumann.

(*Concluded from the November Number*, p. 490.)

[PLATES XV.–XIX.]

I WILL now put forward some details, for it seems necessary that some proofs of what has been maintained in general should be given, and I shall therefore quote some examples of the intimate connexion between earth-structure and terrestrial magnetism. No diagrams could indicate the relations better than Locke's Magnetic Sections across the Hudson, which were taken at three different points of the Palisades, Snake Hill, Fort Lee and Patterson (see Plate XV.). The dip and intensity were in each case determined at a number of points along a line perpendicular to the range of cliffs,

[1] Iles Ponces, 1788, p. 128.

and the curves rise very suddenly to maxima just above the edges of the cliffs, which are composed of columnar diorite. This diorite is widely distributed and lies between masses of Triassic sandstone; the dip is slight and towards the west. On the east side of the cliffs Laurentian gneisses are exposed. The close proximity of formations so far apart in age must be caused by a great fault, and the perpendicular dislocation amounts at least to some thousands of feet. In Locke's opinion the very sudden rise and fall of the magnetic curves are due to the columns of diorite being magnetized by terrestrial induction, but I think there can be no doubt that the great fissure is the chief cause of the sudden changes.

Another beautiful example is afforded by Nipher's "Magnetic Survey of the State of Missouri."[1] The isogonic line of 7° 30′ describes a loop, in the centre of which lies the Pilot Knob, a hill 662 ft. high, and composed of porphyry, porphyry conglomerate, and layers of hard red sandstone. To the east is the knob connected with other porphyry hills. If we remember that the Pilot Knob can be nothing but an old volcano, whose subterranean core passes to great depths, we can well imagine how the hearthlike eruption may deflect electric currents from their direct track, and cause irregularities in the magnetic elements.

Marvellous coincidences between magnetic curves and lines of great geological disturbance are met with in Asia. It seems as if the Himalayan range exerted some controlling influence over the isogonic lines; some of these follow the direction of the mountain chains, and the way in which the 2° 30′ and 3° curves go high up the Brahmaputra valley and then turn back to the S.W. is very striking (see Plate XVI.). Rijckevorsel, the well-known surveyor of the Indian Archipelago, speaking on this subject, says, that "It is as if the land had some power of coercion over the isogonic lines."[2] I venture to submit that it is not the land which exerts this influence, but the great longitudinal ruptures indicated by the submerged mountain range. It may be that in a great portion of Asia the isogones coincide with lines of great geological displacement.

Another illustration in support of our theory may be taken in Europe. The district between the Carpathian Mountains and the Transylvanian "Erzgebirge" forms an almost circular basin; this region was surveyed by Kreil about forty years ago, and by Schenzel in 1875-76.[3] During the interval the isogones have (of course) changed their position, but still the two maps, one for the period

[1] Nipher, F., Magnetic Survey of Missouri. Fifth Annual Report, Trans. St. Louis Ac., vol. iv. No 3, p. 516. Chart of the Magnetic Variation in Missouri, St. Louis, 1880. Sold at Robert Beneck's, St. Louis. Chart of the Magnetic Variation in Missouri (photograph of a plaster chart), St. Louis, 1881.

[2] Rijckevorsel, Report to His Excellency the Minister of the Colonies on a Magnetic Survey of the Indian Archipelago, made in the years 1874-1877. Amsterdam, 1879.

[3] Schenzel, Beiträge zur Kenntniss der magnetischen Verhältnisse im Südostlichen Ungarn, Repert. f. Experimental Physik, f. physikal Technik, f. math. u. astron. Instrumentenkunde, Herausgeg. v. Carl, München, 1877, Bd. XIII. s. 165.

of Kreil's observation, the other for the later epoch, show a most remarkable similarity. To the east, the isogonic lines are parallel to the Carpathians, and they turn round near Kronstadt and Fogarasch. just as the Transylvanian Alps do. The centre of Transylvania is traversed by a loop. We thus see that the isogones present a thoroughly natural form, and that their characteristic features remain unaltered, notwithstanding the secular changes of magnetism. This is certainly a very important result.

A considerable number of other illustrations might be quoted, but I am afraid of digressing too far, and must refer those who take a special interest in the matter to the above-mentioned paper, in which a discussion of all the magnetic surveys hitherto accomplished will be found. Many pages of it are devoted to the results of the Magnetic and Geological Survey of Japan, carried out under my direction during the years 1880–85. Those results are of special importance to our subject, and I may therefore be allowed to submit some observations which have come under my own experience. There is a most remarkable correspondence between the lines of equal declination and the principal lines of geological structure. In general the magnetic lines exhibit striking and unexpected irregularities, and these irregularities are found to be most intimately connected with the abnormal curvatures of the folds. The serious discussion which followed the reading of a paper of mine before the Seismological Society of Japan in 1882[1] showed how far these irregularities were unexpected. For my own part I was convinced from the very beginning of the Geological Survey, at a time when the magnetic data were still scanty, that there must be some connection between the phenomena caused by terrestrial magnetic force and the internal condition of the earth's crust or of the earth itself. With this point in view the magnetic investigations were commenced. In a comparatively short time the general magnetic survey, comprising no less than 200 complete observations at a like number of stations, was accomplished by Mr. Sekino, one of my former topographical assistants. The results are very satisfactory. It will be observed that the course of magnetic lines are influenced by the great transverse cleft called Fossa Magna (see Plate XIX.). This is a great depression, cleft, or fissure, running from the Pacific to the Japan Sea, in which a number of volcanoes have sprung up. Fujinoyame, for instance, the highest mountain of the Japanese Archipelago, is situated in this cross fissure. The Japanese chain consists of a long series of folds, running as a rule in the same direction as the island chain itself, but in the neighbourhood of the Fossa Magna these folds (which were probably raised by a force acting from the continent towards the Pacific) turn back as if they had been stopped by the great wedge of eruptive rocks lying below the Fossa Magna. In Mr. Sekino's Magnetic Map (Plates XVIII. and XIX.) the 5° isogone will be seen to circumscribe the Fossa Magna, for in that vicinity the curve forms a great wave whose crest is upturned

[1] Notes on Secular Changes of Magnetic Declination in Japan, vol. v. of the Trans. Seism. Soc. of Japan.

towards the Japan Sea. This curve, therefore, exhibits the same features as the axial line of the folds, and to a certain extent the isogones and the fold lines coincide. In a paper published in the Proceedings of the Royal Geographical Society,[1] containing a somewhat fuller account of the geology of Japan and of the distribution of magnetism in that country, I have said: "These results open out an entirely new field of research, and I hope that they may be an inducement to a continuation of similar investigations, so that some light may be thrown upon those still very obscure pages relating to the causes of magnetism and to the internal condition of the earth."

Since the publication of the above-mentioned memoir, letters of approval have been received from many well-known authorities, and a number of reviews, by no means unfavourable, have appeared. But in some cases I have been entirely misunderstood, and quite recently a violent attack has been made by Dr. Cargill G. Knott, of Tokio.[2] He says, that an inspection of the results of Sekino's survey, carried out under my direction, convinced him that it would be unsafe to deduce from them any definite conclusion as to the general magnetic characteristics of Japan, and to him it appeared "that the thing to be desired was a new survey, which might be called a preliminary survey of all Japan." These remarks tend to depreciate Mr. Sekino's observations, and are accompanied by several unfounded accusations. The latter I hope to prove unjustified by stating a number of facts, and as evidence that Mr. Sekino's survey is not to be despised, I have prepared several maps as follows:

Plate XVIII. is a combination of Mr. Sekino's and Dr. Knott's maps, from which it will be seen that there is no essential difference between the lines obtained by the two observers. If they do not exactly coincide, it must be borne in mind that the two maps relate to different epochs (Sekino, 1882–83; Knott, 1887), separated by four or five years. In discussing the question of secular change, Dr. Knott ignores the fact that the rate of change varies with time *and locality*, hence his method of comparison is unsatisfactory, and average differences are not allowed. Apart from adverse statements, I do not hesitate to welcome Dr. Knott's work, for his map is a good check on Mr. Sekino's observations, and shows that Sekino's survey *can stand* as a Preliminary Survey of Japan.

Plate XVII. is a combination of Dr. Knott's map with my Tectonic Map of Japan. Speaking of his own map Dr. Knott says that "two regions are to be noted as presenting magnetic irregularities; one is the great mountainous region about the Fossa Magna, and the other is between the 38th and 40th parallels. I quite agree with Dr. Knott even as regards the irregularities of Northern Honshin. They are mentioned in my paper on the Geology of Japan, in which I

[1] The Physical Geography of Japan, Proc.R.G.S. Feb. 1887.

[2] Cargill G. Knott, D.Sc. (Edin.), F.R.S.E., Professor and Aikitsu Tanakadate Professor of Physics, Imp. University of Japan, A Magnetic Survey of all Japan, carried out by Order of the President of the Imp. University, Journal of the College of Science, Imp. University Japan, vol. ii. part iii. Tokio, 1888.

make the following remarks: A kind of break of the isogone occurs between Sado and Sendai. This must be of special interest, as a geological dislocation line runs across Japan in this region. The irregularities are also referred to in a later pamphlet on magnetism. As regards *Dr. Knott's curves*—although some details of declination may be questioned—I think they *are a sufficient proof that the distribution of magnetism bears some relation to the great lines of geological structure.*[1]

When I undertook the Geological Survey of Japan, of which the magnetic survey was part, the steps taken were guided by previous experience.

The bulk of mariner's observations were gone through, and Ino's field books and maps consulted for compass-measurements. These studies enabled me to select the stations at which Mr. Sekino's observations were subsequently taken, in accordance with a definite scheme.

The Sado sinuation is roughly indicated on a small map published in 1882, and from my magneto-tectonic map (Plate XIX.) it will be seen that the selected stations keep fairly close to the ascertained magnetic curves. These facts are sufficient to show that the arrangement of stations was made with the utmost care. I am afraid that Dr. Knott's stations were not distributed so judiciously. It is greatly to be regretted that the second magnetic survey of Japan was carried out so independently, and without the valuable hints which might have been derived from previous investigations. Not even the field books of the Geological Survey were consulted. These books contain sketches by which the exact position of every station can be easily determined, and in accordance with the practice of Lamont the topographical data necessary for the redetermination of the places of observation were ascertained, in order that future observers might work on the same spots.

From the memoir on the second Magnetic Survey of all Japan we learn that of the 81 stations taken 27 *can be regarded* as coincident with stations of the previous survey. A few of these are only roughly coincident. I should digress too far by stating fully the reasons why I determined that the stations should be on the route followed by Messrs. Sekino and Kodari; it is sufficient to say that the test offered by the second Magnetic Survey justifies their distribution. At the same time, I cannot avoid stating that the new observers should have felt it their duty to devote special attention to the most interesting regions about the Fossa Magna, and between Sado and Sendai. Dr. Knott considers it very important to fairly distribute the stations over the whole country, and that it is necessary "to give volcanoes a wide berth, as these have been shown by previous observers to be a great source of disturbance,

[1] There is something very striking in Dr. Knott's representation. The centre of the great loop of 4° 20′, which will be noticed in the main island, coincides with the intersection of the great longitudinal and transverse fissures (Fossa Magna) of Japan. His curve of equal total force, and that of equal horizontal force, also prove that the Fossa Magna has some influence on the magnetic curves.

especially as regards the declination." Then, almost in the same breath, he searches for the causes of observed irregularities, and finds them in the distribution of volcanic rocks. Little or no attention seems to have been paid to the most interesting disturbance that must have existed at Ino's time (at the beginning of this century), and which Mr. Sekino's observations show to still exist, nor was the peculiar phenomenon exhibited by the magnetic block on the summit Morigoski examined; this is all the more surprising, as two stations of Dr. Knott's survey lie in the immediate neighbourhood of these remarkable places.

It may appear that I am dealing too minutely with Dr. Knott's paper, but I think the criticisms are justified by the interests of science. It is by no means satisfactory to see that so many fellow-workers on the field of science cannot build without pulling down, for it should be the serious duty of every one to advance from wherever a safe footing has been attained. Earnest and honest labour deserves respect rather than condemnation. Unfortunately it is usual for critics to introduce their subject with an exhaustive survey of all its faults, instead of commending its merits. The greater the effort made to ascertain its merits and appreciate them, the easier will it be to use existing knowledge as a foundation for future progress, and those who neglect this duty tend to check rather than promote the advancement of science.

Dr. Knott does not appear to be acquainted with my pamphlet, for if he had read it he need not have devoted so many pages to the rather antiquated chapter on Rock Magnetism; and if his knowledge of the Geology of Japan was more profound, his opinion as to the causes of irregularities being entirely superficial would be modified. In his paper we are taught that "the volcanic nature of the rocks is more than enough to account for all irregularities." Why? Perhaps because volcanic rocks are known to contain magnetic iron ore? If so, Dr. Knott should have remembered that the volcanic tuffs, widely distributed over the country, consist of the same materials as so-called volcanic rocks. In addition, as proved in my memoir, rock magnetism does not influence the curve-systems, nor do the values of the constants depend on the nature of the rocks developed in the neighbourhood.

In considering such questions the following points are important: how the earth's crust is composed of *masses* of rocks; how deep the fissures penetrate into the earth and establish connection with the interior of the globe; whether these fissures are closed or not, etc. In the locality under discussion there are two enormous fissures, intimately connected with the development of the great earth-wave whose crest appears as the Japanese island chain. One of these is longitudinal, and divides the whole mountain-range into inner and outer zones; the other is transversal, and divides the chain into two parts which stretch towards the north-north-east and west-south-west respectively. The transversal fissure is indicated by the Fossa Magna, and the longitudinal one curves back towards the Sea of Japan, where it intersects the former (see Plate XVII.). Enormous

masses of molten matter have been ejected from these clefts, and it is not difficult to imagine that such enormous fissures have some influence upon earth currents, and consequently on the magnetic needle.

My former remarks on the reduction of the declination values to one hour are said to contain a "slight" inaccuracy, and Dr. Knott seems to think that no correction was applied to observations made at the northern stations. On this supposition he depreciates Sekino's observations, and, together with the supposed injudicious distribution of stations, it is used as an argument against the first Magnetic Survey.

The title of Sekino's Magnetic Map of Japan, which was exhibited at the Berlin International Congress of Geologists, and now belongs to the Geographical Society of Berlin, runs as follows, "On trouve les valeurs actuelles des observations indiquées près des stations par les mêmes coulers que les coleurs correspondantes. Les observations sur la declinaison seulement sont réduites en moyenne diurne." Before publishing my pamphlet on magnetism I wrote to Sekino asking him to give me the exact times at which each observation was taken, and he replied that the values in the list I kept were daily means. I am certain that before leaving Japan I gave instructions for a reduction of all declinations to be made, and hitherto I was convinced that this had been done, although the reduction for the northern stations could only be approximate. If Dr. Knott has any doubts about the matter, he may readily remove them by inspecting Sekino's field books, for, as stated above, these books were not written to be buried in the Archives of the Geological Survey, but were prepared for the use of future observers.

There are, I think, very few persons who have a good idea of the surface configuration of the earth, even where only a small part is concerned. In most cases heights are exaggerated in memory, and consequently the angles of inclination are taken far too great. How insignificant is the depth of the deepest part of the ocean when compared with the dimensions of the globe as a whole! Lamont has tested the question as to whether there is any change in the magnetic elements due to differences of level, and the results show the change to be imperceptible even for considerable differences of height. From this he concluded that the seat of terrestrial magnetism is not to be sought near the surface, but at enormous depths below the surface.

I may here be allowed to draw attention to a most interesting theory of Lamont. He admitted the frequent occurrence of irregularities in the systems of magnetic curves, and attributed them, not to the influence of magnetic masses contained in the crust, but to elevations and depressions, *i.e.* to irregularities, of the earth's nucleus.

His opinion may best be stated in his own words: "The magnetic curves represent the surface of the nucleus." If Dr. Knott had been well acquainted with these remote but classical investigations, he might have omitted the discussion of results given on page 214 of his Memoir.

In my pamphlet on Magnetism I have given a fairly complete

list of the most important works on the subject, and I consider it necessary that every scientific investigator should have a profound knowledge of the literature of his subject; for when so qualified he need not discuss works which he has not studied carefully.

Amongst the works which I have had occasion to peruse during my studies in terrestrial magnetism, a memoir by C. A. Schott on the secular variation of magnetic declination in the United States and at some foreign stations, is of special interest. Any one who has devoted any attention to the subject of secular change, knows that it is one of the most complicated branches of terrestrial physics, and at first sight it seems very curious that by far the greater number of copies of the above memoir were sold to lawyers. The demand in law circles for the work which treated of a purely scientific subject was so great, that several new editions had to be issued; and the reason of this is to be found in the difficulties experienced in following the old boundary lines of landed property, which had been originally laid out by the magnetic compass. This case is a good example of how a purely scientific problem may suddenly and unexpectedly become one of great practical importance.

Schott's investigations tend to show that the secular variation is periodic, but unfortunately the observations do not extend over a complete cycle. He compares the secular change with the motion of a pendulum. In the United States south of the 49th parallel, a complete cycle requires $2\frac{1}{2}$ to $3\frac{1}{2}$ centuries, and during this interval the needle describes arcs varying from 3° to 7°; in Paris the amplitude is 33° and the period about $4\frac{2}{3}$ centuries.

Taken as a whole, the secular variation is perfectly systematic, and subject to remarkable laws which will well repay careful study, and being a phase in the life-history of our planet, the secular change may prove an excellent means of comprehending the internal condition of the globe.

In Japan the magnetic elements have undergone considerable changes since the beginning of the present century, as may be seen by comparing recent surveys with that of Ino Tadagoski, a Japanese astronomer, who, about that period, made a geodetic survey of his native country. This observer had heard and read in foreign books of the variation of the compass, but, nevertheless, he denied its existence, and even went so far as to attribute the declination observed by Europeans to errors in their compasses; he also maintained that the fact of his own needles always pointing due north, was owing to the superiority of his instruments, which were of his own construction. Thus he was led to believe that no variation of the compass existed. The average amount of secular change during the interval 1800 to 1880 is between 3 and 4 minutes of arc per annum. Dr. Knott maintains that at the present time there is no secular change in Japan, and if this be correct, it is an important and unexpected result. Let us see how far it may be relied upon.

As stated above, Dr. Knott uses a method which is a *contradictio in adjecto*, for if an average of differences be taken, and this average proves to be zero, as in the case considered, it does not necessarily

follow that there is no secular variation in Japan, but the result can be explained by the well-known fact that the amount of secular change is different in different places, and may be positive in some, whilst it is negative in others.

From a comparison of Sekino's and Knott's maps, however, it may be seen that the isogonic lines have shifted very little indeed, and we agree with Dr. Knott when he says: "Within the period beginning with 1883 and ending 1887, there is practically no change, or if there is, it is a very small change indeed. It looks almost as if we were just passing through a time of maximum declination." This result is extremely interesting if we take into account what Fritsche has said about the line of zero declination when it passes through Mongolia. The value of the secular variation for stations lying near this part of the line is very small, and we may therefore conclude that the whole magnetic island of western declination covering a considerable part of Eastern Asia is at present in a nearly stationary condition.

When speaking of the diurnal means obtained from Sekino's survey, I quoted part of the explanation accompanying his magnetic map, and I now direct attention to another passage of that explanation, which runs as follows: "Il y a un nombre d'irrégularités considérables qui sont particulières à des régions isolées. Ce ne'st pas le caractère spécial de ces irrégularités qui est représenté dans la carte par les lignes en forme de cercle, mais simplement le lieu et l'extension." There is no doubt but that a complete magnetic survey of Japan will prove the existence of many small magnetic islands, which can at present only be vaguely indicated.

Dr. Knott's map does not show the Sado sinuation of the south-west curve as I have shown it, but represents the irregularity by an isolated line round the island. He prefers this form because—as he says—it is well known that the isogonic lines at or near islands often present irregularities of quite a local character. I am inclined to believe that this is not well known. I, at least, was not aware of it, except as regards oceanic islands which may —as Rijckevorsel says—appear to have some power of coercion on the isogonic lines. Oceanic islands, however, are indications of great submarine mountain ranges, which are generally accompanied by deep fissures in the earth's crust, but no power of coercion can be expected from an island which is merely a detached piece of the main land.

The problems presented by the magnetic islands of Japan can only be solved by a very detailed survey, and it is interesting to enquire what such a survey means. Quite recently a very detailed magnetic survey of part of the kingdom of Würtemberg has been carried out by Hammer, and he estimates that about 90 stations would be necessary for the whole kingdom. Taking this as a basis, Japan would need 1800 stations, Germany 2500, and Great Britain 1500 stations.

These numbers induce me to again refer to the distribution of stations in the first Japanese survey, objected to by Dr. Knott. In

his survey observations were taken at 80 stations, and in the first survey 200 stations were selected. When compared with the requirements of a detailed scheme, both these numbers appear very modest. In such cases it is all the more necessary to distribute the stations judiciously instead of merely spreading them "fairly over the whole country," for it is only exhaustive surveys which can adopt the latter principle advocated by Dr. Knott. Similar remarks may apply in orographic surveys, for a reconnaissance requires quite a different distribution of stations to that needed for an elaborate undertaking in which time and money are not important. Having had some experience in reconnaissance surveying, I am well aware how carefully the stations must be selected.

The above numbers also give us an idea how very far we still are from having accomplished detailed magnetic surveys even of parts of the earth's surface; it is only in a few very limited districts in Missouri, Transylvania and Würtemberg that such surveys have been attempted. The whole question needs to be taken up internationally, and I trust that these lines may convince my readers of the great importance of the subject. Rich harvests may be expected from a field which can only be explored by the joint efforts of all the workers in this great cause, and the stimulus imparted by British scientists will be sure to find a ready response on the continent.

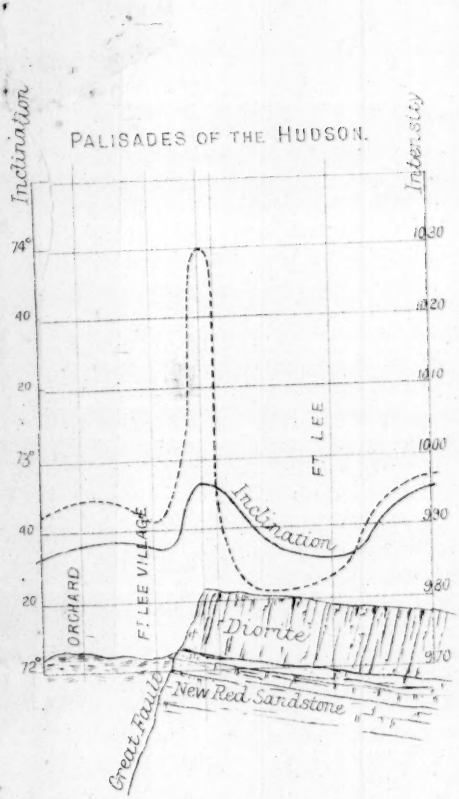
To many sciences the problem of terrestrial magnetism is the common focus, for physicists, geographers, topographers, meteorologists, astronomers, etc., and above all geologists, must be interested in it. In former times magnetic observations were commonly attached to astronomical ones, and astronomers were in charge of the observations, whilst at present the subject forms a branch of physiography, and the observatories are usually connected with meteorological establishments. The latter arrangement seems to be the most satisfactory one, and great interest is taken in the question by meteorological societies and offices on the continent, particularly at Hamburg, Berlin, Vienna and Munich.

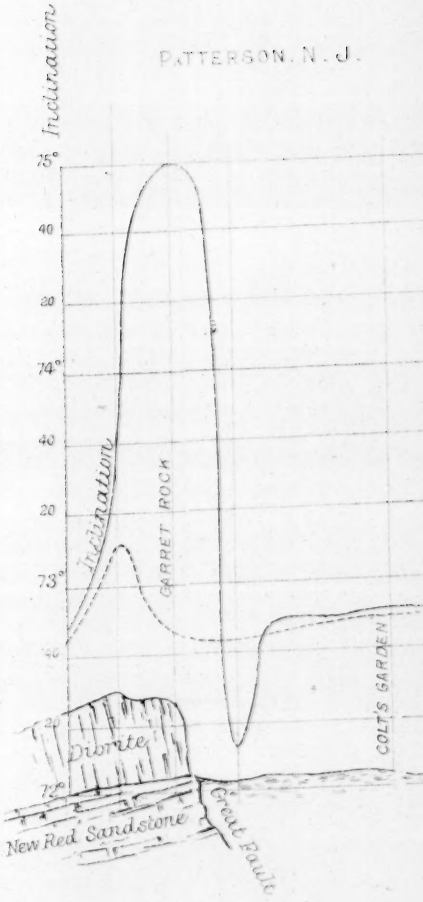
To come to the practical side of the question, I beg to propose that an International Congress be held, say in London, at or about the time of the next British Association meeting. This Congress might decide numerous questions, still unsettled in the minds of the majority of observers, and may pass resolutions prescribing uniform schemes on which magnetic surveys should be carried out. It should also endeavour to induce the various Governments to take an active part in promoting the magnetic survey of their respective countries.

Dr. Naumann's paper is illustrated by Plates XV.–XIX.

Plate XV. Locke's Magnetic Sections; Palisades of the Hudson and Patterson, New Jersey.
Plate XVI. Magnetic Curves of the Himalayas.
Plate XVII. Tectonic Map of Japan with Dr. Knott's Magnetic Curves.
Plate XVIII. Comparative Diagram of Mr. Sekino's and Dr. Knott's Magnetic Curves of Japan.
Plate XIX. Magnetic Tectonic Chart of Japan by Dr. E. Naumann.

LOCKE'S MAGNETIC SECTIONS.





To illustrate Dr Naumann's Memoir on Terrestrial Magnetism.

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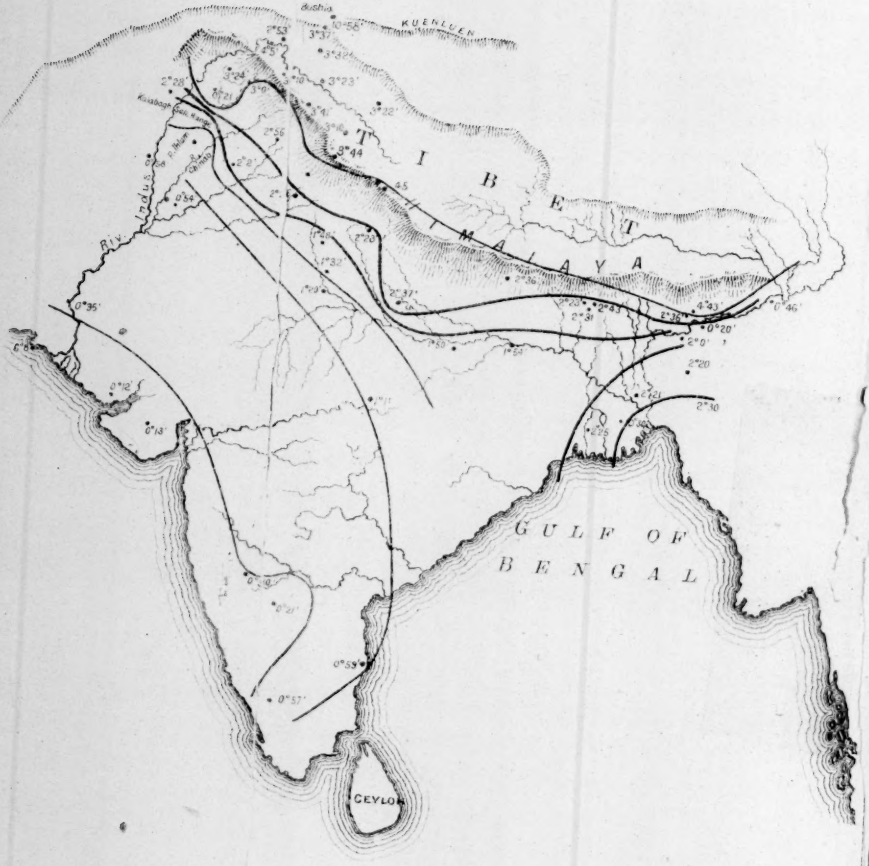

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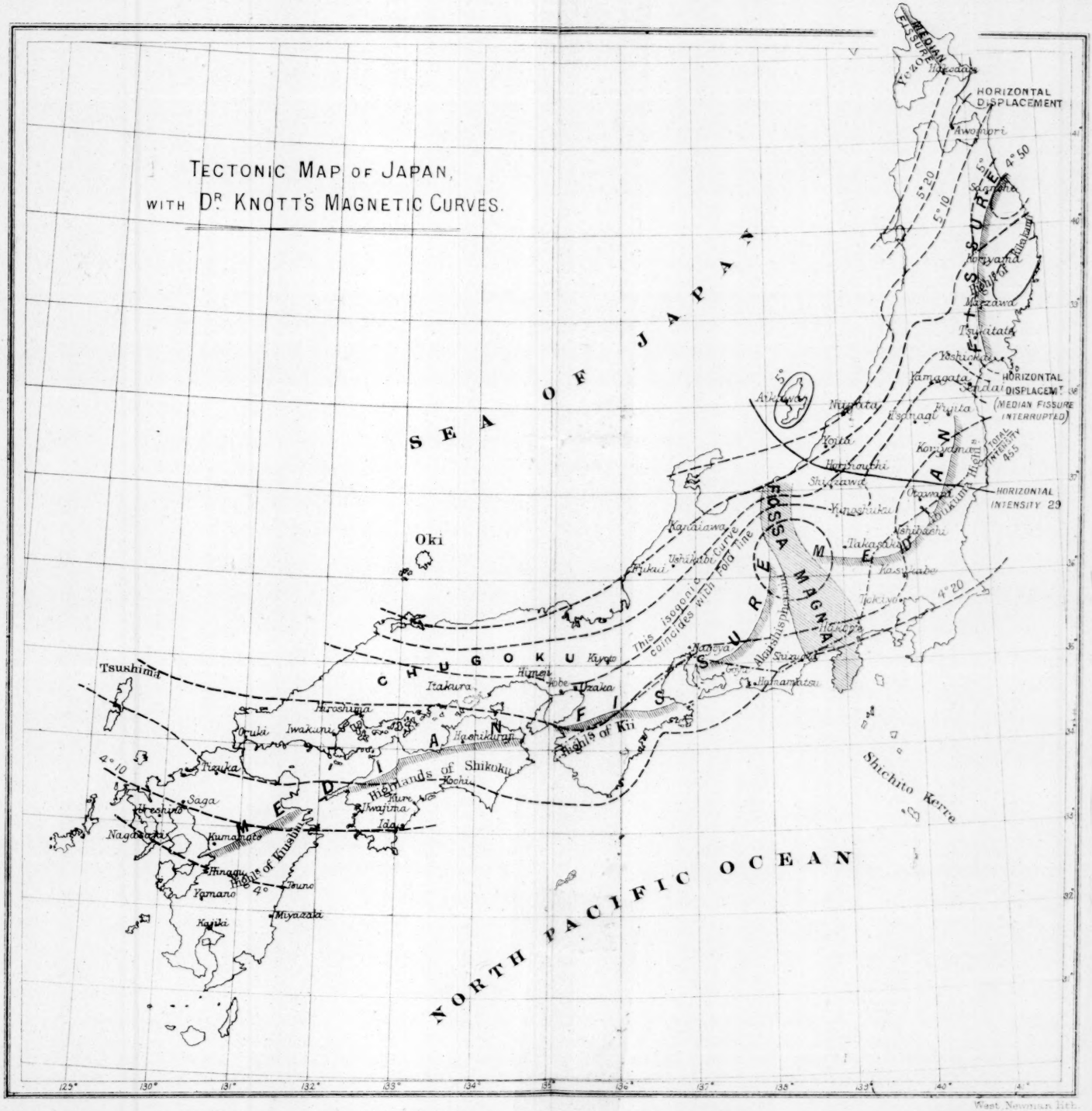
To illustrate Dr Naumann's Memoir on Terrestrial Magnetism.



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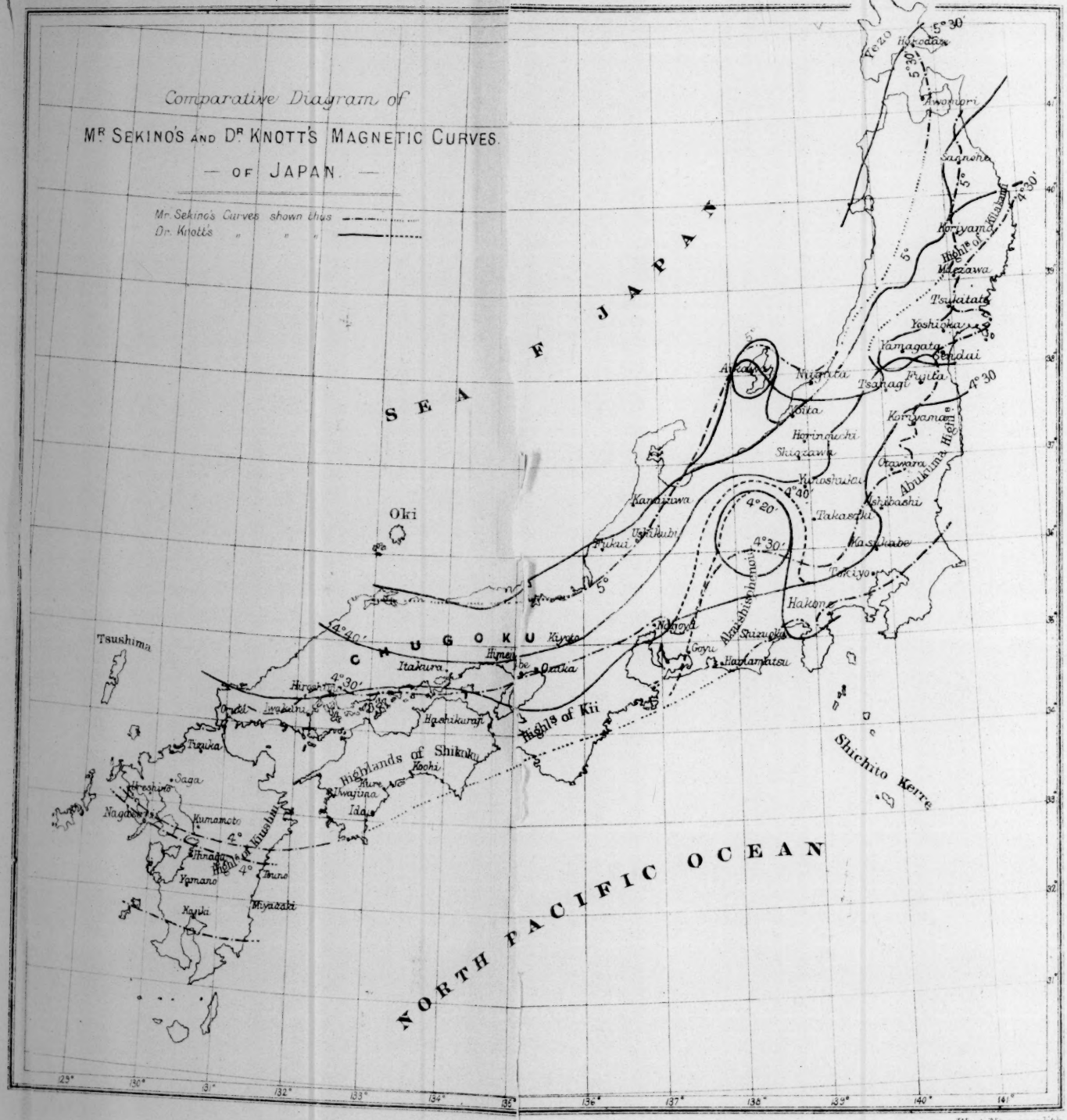

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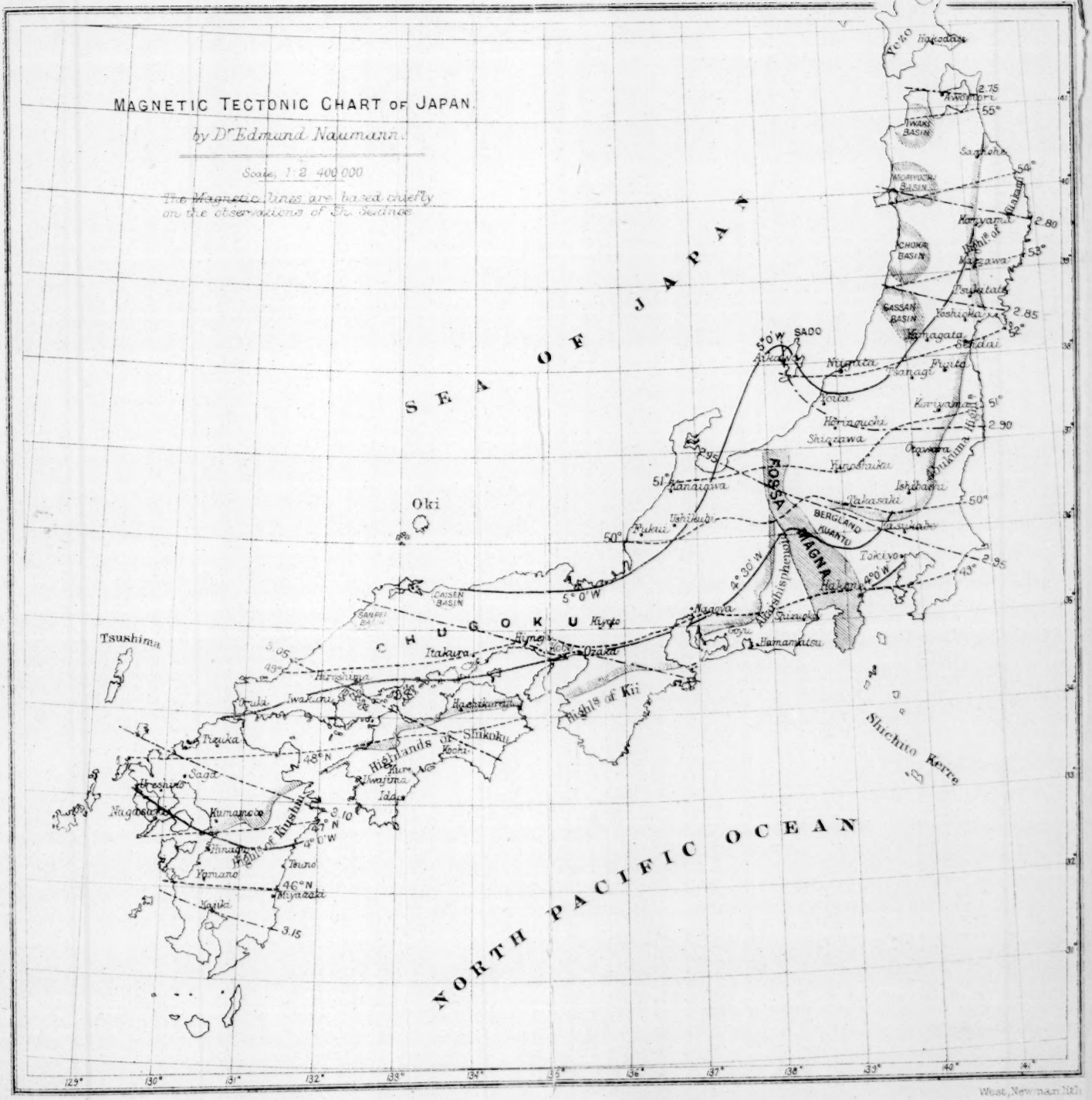
To illustrate Dr. Naumann's Memoir.



West, Newman lith.

To illustrate Dr Naumann's Memoir.

III.—On the Lower Silurian Felsites of the South-East of Ireland.[1]

By Frederick H. Hatch, Ph.D., F.G.S.

(Communicated by permission of the Director-General of the Geological Survey.)

THE felsites of the south-east of Ireland are shown on the maps of the Geological Survey [2] to extend over considerable areas in counties Wicklow, Wexford and Waterford. Like the Welsh felsites they are contemporaneous with Lower Silurian (Ordovician) strata, and were probably erupted on an old sea-bottom. They are accompanied by abundant deposits of tuffs and breccias, the component fragments of which consist mainly of felsite.

A chemical examination of one of the felsites, made when I was preparing a petrographical description of the more important igneous rocks for a memoir on Sheets 138 and 139 of the Irish Geological Survey, showed it to be a soda-felsite;[3] and this discovery induced me to make a more extended examination of these felsites. A number of specimens from different localities were collected and sliced; and a chemical analysis of those that promised interesting results was kindly undertaken by Mr. J. Hort Player (to whom I here tender my best thanks). The object of this paper is to communicate these results and to discuss them in connection with the microscopic characters of the rocks.

The main point brought out by chemical analysis is the almost entire absence of lime, and the presence of potash and soda in varying proportions: in other words, the lime-soda series of felspars is unrepresented in these rocks, which must therefore contain either a varying proportion of potash-felspar (orthoclase) and soda-felspar (albite) or one or more felspars of a potash-soda series (anorthoclase).

According to the relative proportion of potash and soda the rocks are roughly separable into the three following groups:

(I.) Those in which there is a large excess of potash over soda, the latter being present only in small quantity. These may be termed *potash-felsites.*

(II.) Those in which the soda, though present in considerable quantity, is yet subordinate to the potash. These may be termed *potash-soda-felsites.*

(III.) Those in which the soda is in excess. These may be termed *soda-felsites.*[4]

[1] Read at the Newcastle Meeting of the British Association, Sept. 13th, 1889.

[2] Sheets 130, 139, 148, 149, 158, 167, 168, 169, 178, and 179.

[3] Geol. Mag. 1889, p. 70.

[4] Should the soda be in but slight excess of the potash, the rock might be termed a *soda-potash-felsite.* The term *keratophyre* (originally suggested by Gümbel) has been applied by Lossen to a rather indefinite group which includes rocks similar in character to those in Group III. The term soda-felsite appears more applicable to these rocks.

GROUP I. (*Potash-felsites.*)[1]

	1. (I. 113.)	2. (I 172.)	3. (I. 110.)	4. (I. 111.)
SiO_2	70·8	71·0	66·2	85·2
Al_2O_3	15·1	14·2	13·6	7·4
Fe_2O_3	1·0	·8	3·3	·5
FeO	·6	·7	4·6	·4
CaO	·2	trace	trace	trace
MgO	1·1	1·1	1·8	·4
K_2O	9·1	9·6	7·9	4·4
Na_2O		·7	·1	·9
Loss on ignition	1·6	1·5	2·6	·6
Total	99·7	99·6	100·1	99·8
Sp. G.	2·606	2·606	2·619	2·622
Orthoclase percentage[1]	54·2	57·1	47·1	26·2
Albite percentage	0	5·9	·8	7·6

GROUP II. (*Potash-soda-felsites.*)[2]

	5. (I. 81.)	6. (I. 126.)	7. (I. 169.)	8. (I. 109.)
SiO_2	70·6	74·8	75·6	73·6
Al_2O_3	15·3	13·9	12·8	13·8
Fe_2O_3	·7	·5	—	·5
FeO	1·7	1·0	1·9	2·4
CaO	·8	trace	·1	·6
MgO	·8	1·1	·4	·7
K_2O	6·1	5·7	5·6	4·3
Na_2O	2·7	2·2	3·0	3·2
Loss on ignition	·9	1·2	·5	·6
Total	99·6	100·4	99·9	99·7
Sp. G.	2·645	2·600	2·626	2·658
Orthoclase percentage	36·4	33·9	33·3	25·7
Albite percentage	23·8	18·6	25·4	27·1

GROUP III. (*Soda-felsites.*)[3]

	9. (I. 83.)	10. (I. 80.)	11. (I. 114.)	12. (I. 96.)
SiO_2	73·0	71·2	77·8	71·6
Al_2O_3	16·7	16·8	13·2	16·9
Fe_2O_3	·5	·8	·2	·3
FeO	1·3	1·5	·7	·9
CaO	·5	·8	trace	2·2
MgO	·7	·9	trace	·6
K_2O	2·2	2·0	2·1	1·3
Na_2O	4·2	4·7	5·1	4·7
Loss on ignition	1·2	1·5	·6	1·1
Total	100·3	100·2	99·7	99·6
Sp. G.	2·607	2·606	2·634	2·607
Orthoclase percentage	13·0	11·9	12·5	8·5
Albite percentage	35·6	39·9	43·2	39·9

[1] The whole of the potash has been calculated as orthoclase from the formula $K_2O.\ Al_2O_3.\ (SiO_2)_6$; and the soda as albite from the formula $Na_2O.\ Al_2O_3.\ (SiO_2)_6$.

[2] See also three analyses of felsites from the counties of Wicklow, Wexford, and Waterford by the Rev. S. Haughton (Trans. Roy. Irish Acad. vol. xxiii. part ii. 1859, p. 615).

[3] See also two analyses of soda-felsites from the Waterford coast by the late J. A. Phillips (Phil. Mag. 1870, vol. xxxix. p. 12; GEOL. MAG. June, 1889, p. 288), and an analysis of a Wicklow soda-felsite by the author (GEOL. MAG. Feb. 1889, p. 70).

Notes on the foregoing Groups.

Group I.—1. (I. 113.)—Half a mile south-east of Cairn on Castletimon Hill, Co. Wicklow. Sheet 130.

Compact grey felsite, of irregular fracture. Under the microscope shows felsitic (cryptocrystalline) structure, but with frequent patches of more coarsely crystalline (microcrystalline) matter. No porphyritic crystals.

2. (I. 172.)—One mile west of Great Newtown Head, Co. Waterford. Sheet 179.

Compact brown felsite, of irregular fracture. Under the microscope shows a cloudy unindividualized substance, containing a number of ill-defined spherular clots, mostly aggregated in strings. These bodies have no radially fibrous structure, presenting between crossed Nicols a speckled or dappled depolarization instead of the characteristic black cross, and cannot therefore be regarded as true spherulites. They doubtless represent, however, some incipient form of crystallization. The remainder of the ground-mass shows a confused, patchy devitrification. The rock contains no crystals and is probably a devitrified pitchstone.

3. (I. 110.)—Castletimon-ford, right bank, Co. Wicklow. Sheet 130.

Compact, grey, mottled felsite, fracturing easily; the joint-faces stained with oxide of iron.

Felsitic or cryptocrystalline structure with an occasional porphyritic crystal of non-striated felspar (orthoclase). Between crossed Nicols the devitrification of the ground-mass shows itself in the presence of innumerable minute spherular bodies, each giving a black cross. Chlorite is present in scattered scales.

4. (I. 111.)—Castletimon-ford, left bank, Co. Wicklow. Sheet 130.

Very compact, light grey felsite; brittle and splintery, fracturing easily along joints, the faces of which are stained yellow with oxide of iron, resulting from the decomposition of pyrites which occurs in small quantities in this rock.

Under the microscope the structure is truly felsitic or cryptocrystalline, there being no porphyritic crystals present.

Group II.—5. (I. 81.)—Quarry on road from Woodenbridge to Aughrim, $1\frac{1}{4}$ mile north-west of Woodenbridge, Co. Wicklow. Sheet 139.

Compact grey felsite, spotted white with small crystals of felspar. Iron pyrites in disseminated specks.

Under the microscope: Cryptocrystalline ground-mass, embedding numerous porphyritic crystals of striated felspar. The twinning is on both types (pericline and albite). Microperthite structure is occasionally shown.

6. (I. 126.)—Kilmacrea Wood, $1\frac{3}{4}$ mile north-west of Redeross, Co. Wicklow. Sheet 130.

Compact, grey, mottled felsite.

Under the microscope: Structure varying between cryptocrystalline, in which the constituent granules are not distinctly separable, and microcrystalline in which each granule can be clearly distinguished. Here and there occur a few larger grains of quartz and felspar, but not sufficiently separated from the ground-mass to constitute a true porphyritic structure.

7. (I. 169.)—Boulder on coast, near Annstown, Co. Waterford. Sheet 178.

Compact dark grey felsite, well banded (fluidal structure).

Under the microscope: Felsitic ground-mass with confused and patchy devitrification. Embedded in the ground-mass are large porphyritic crystals of quartz and felspar, the former corroded, the latter presenting both single and double twin-striation.

8. (I. 109.)—One mile south-east of Ballynacor Cross-roads, Co. Wicklow. Sheet 130.

Compact back felsite; fracture irregular.

Under the microscope: Confused felsitic ground-mass, depolarizing in patches, with isolated porphyritic crystals of striated felspar.

Group III.—9. (I. 83.)—Quarter-of-a-mile east of Coatsbridge, on the road from Woodenbridge to Aughrim, Co. Wicklow. Sheet 139.

A grey almost phanerocrystalline felsite, with slight parallel structure (probably secondary and due to the earth-movements that have affected this region).

Viewed between crossed Nicols the ground-mass has an intricate mosaic-like

appearance produced by the uniform distribution of minute prisms of felspar among the rounder granules of quartz. The porphyritic felspar, which is not abundant, occurs in broad rather irregular grains, presenting sporadic twin-striation.

Scales, shreds and streaks of both muscovite and chlorite are rather abundant.

10. (I. 80.)—Half-mile north-west of Woodenbridge, on the road from Woodenbridge to Aughrim, Co. Wicklow. Sheet 139.

A grey almost phanerocrystalline felsite, like No. 9.

Under the microscope: Microcrystalline ground-mass, with porphyritic grains of quartz and felspar, the former much corroded and rounded, the latter showing occasional twin-striation.

11. (I. 114.)—Ridge, immediately south-east of cairn on Castletimon Hill, Co. Wicklow. Sheet 130.

Compact felsite, of light-grey colour, but with dark mottlings.

The micro-structure of this rock is extremely varied: at one place cryptocrystalline, presenting the characteristic dappled "felsitic" appearance between crossed Nicols; at another, almost coarsely microcrystalline, the grains of felspar and quartz being clearly distinguishable. The structure is, however, nowhere truly porphyritic.

12. (I. 96)—Old Quarry, Little Rock, Arklow, Co. Wicklow. Sheet 139.

Light-coloured felsite, spotted white with porphyritic crystals of felspar.

Under the microscope this rock is seen to consist of fairly large porphyritic crystals of quartz and felspar embedded in a microcrystalline ground-mass. The latter is made up of a uniform minutely granular aggregate of quartz and felspar, together with minute flakes and scales of muscovite and chlorite.

The quartz occurs in round and corroded grains, of which two or three are often closely aggregated. The felspar is present in broad rectangular crystals, presenting good crystallographic contours. The crystals occasionally show dual twinning; more frequently, however, the twinning is polysynthetic, the crystals being striated either in one direction alone (albite type) or in two directions at right angles to one another (albite and pericline types). A slight development of zonal structure is here and there to be observed. In no case was the extinction-angle found to exceed 20°.

Group I. comprises felsites containing few or no porphyritic crystals; they are composed mainly of a truly cryptocrystalline or felsitic aggregate. The few porphyritic crystals are not striated, and consist doubtless of orthoclase.

Groups II. and III. embrace felsites in which a striated porphyritic constituent is more or less abundant. On the other hand, the felspar of the ground-mass can be safely referred to orthoclase, as in Group I. And it seems likely that the fluctuation in the percentage of potash and soda in these rocks may be due to a variation in the relative proportion between a porphyritic albite-felspar and the orthoclase-felspar of the ground-mass. It is not impossible, however, that the porphyritic felspars may belong to a triclinic potash-soda series (the anorthoclase series of Rosenbusch[1]), analogous to the soda-orthoclase or soda-microcline described by Förstner[2] in the liparites of Pantellaria, by Brögger[3] in the augite-syenite of Southern Norway, and by Miers and Fletcher[4] from Kilimanjaro. In all these cases, however, the analyses show the presence of 2 or 3 per cent. of lime, which, with the notable exception of the Little Arklow Rock (No. 12), is almost entirely absent from the Irish soda-felsites. Unfortunately the felspar-grains in the latter are too small for

[1] Mikros. Physiog. vol. i. (1885), p. 550.
[2] Zeitsch. f. Kryst. vol. viii. (1884), p. 125.
[3] Die Silurischen Etagen 2 and 3, 1882, p. 261.
[4] Min. Mag. vol. vii. (1887), p. 131.

mechanical separation with a view to chemical analysis. Isolation with the Borotungstate solution was indeed tried by Mr. Player, but without success. Lossen[1] states that the porphyritic crystals, in the keratophyres described by him, are, in great part, mechanical admixtures of orthoclase and albite, similar to microperthite; but in the rocks under consideration I have been able only in quite isolated instances to record the presence of micro-perthite structure.

The rocks embraced in Group III. are slightly more crystalline than the others, the ground-mass being in general microcrystalline instead of cryptocrystalline. No. 12 (Little Arklow Rock) indeed has been placed by Haughton[2] among his soda-granites; but the structure of this rock can scarcely be termed granitic; at most it could only be called microgranitic (in Rosenbusch's sense of the term), since it consists of porphyritic crystals imbedded in a microcrystalline ground-mass.

It only remains to point out that the modern equivalents of these ancient felsitic lavas are the rhyolites or liparites and pantellerites, which have been subdivided by Rosenbusch[3] into potash-liparites, soda-liparites and pantellerites, sanidine being the porphyritic constituent characteristic for the first, albite for the second, and anorthoclase for the pantellerites. The main difference between the liparites and the felsites lies in the character of the ground-mass, the glassy base of the former being replaced in the latter by the cryptocrystalline aggregate known as *felsitic matter*.[4]

Some of the rocks, especially those of Group I., show indications of having consolidated as true glasses (pitchstone or obsidian), as has been proved to be the case in several instances among the felsites of Shropshire and Wales by the valuable researches of S. Allport[5] and F. Rutley,[6] who have succeeded in detecting distinct traces of perlitic and spherulitic structure in these rocks. The Welsh felsites are of the same age (Bala) as the Irish rocks, and probably belong to the same series of volcanic outbursts.

IV.—Physiography of the Lower Trias.[7]

By T. Mellard Reade, C.E., F.C.S., F.R.I.B.A.

Introduction.

THE origin of the Triassic rocks of Britain is a question that has excited from time to time much interest and varied speculation. The entire absence of fossils in the Lower Trias or Bunter Sandstone has led the majority of geological reasoners to look to causes

[1] Jahrbuch d. k. Preus. Geol. Landesanst. für das Jahr 1884 (1885), p. xxxii.

[2] Trans. Roy. Irish Acad. vol. xxii. pt. ii. (1859), p. 609.

[3] Mikros. Phys. vol. ii. 1887, p. 528.

[4] Nothing of the nature of *microfelsite* (in Rosenbusch's sense) has been observed by me during the examination of these rocks.

[5] On the Pitchstones and Perlites of the Lower Silurian District of Shropshire, Q.J.G.S. vol. xxxiii. (1877), p. 449.

[6] On Perlitic and Spherulitic Structures in the Lavas of the Glyder Fawr, N. Wales, Q.J.G.S. 1879, p. 508; and in Memoir of Geol. Survey on the Felsitic Lavas of England and Wales.

[7] Read at the Brit. Assoc., Newcastle-on-Tyne, in Section C. (Geology), Sept. 1889.

other than marine action for an explanation of its characteristic features. The late Mr. Godwin-Austen was of opinion that the whole of the Triassic rocks were laid down in freshwater lakes which, passing through the brackish stage, become finally saturated with saline matter through evaporation exceeding the inflow of fresh water. It has been felt, however, by other geologists, that this theory, while accounting fairly well for the upper deposits of the Triassic age, does not fit in with the phenomena of current bedding, the presence and distribution of the numerous quartzite and other well-rounded pebbles, and the comparative absence of marl-beds, which distinguish the Lower Trias or Bunter Sandstones.

Hence some well-qualified observers, following Professor Bonney,[1] prefer to consider that the mass of the sandstones have been laid down by rivers and point to Central Asia, as a region where the same accumulations are now taking place. The late Mr. John Arthur Phillips suggested that much of the sandstone displaying the well-rounded or "millet-seed" grain, supposed to be the distinguishing characteristic of the upper and lower beds or divisions of the Bunter Sandstone, was accumulated by wind action, it being maintained that the individual grains could not have been worn to so spherical a form by any other agent.[2] A combination of these two views seems to constitute the prevailing if not the finally accepted theory of the origin of a large proportion of the Triassic Sandstones of Britain.

My attention, which has for a long time been attracted by these interesting speculations, was some time since more specifically directed to the subject. Having for engineering and other purposes to examine considerable areas of the Triassic deposits of the North-west of England, the opportunity of further prosecuting the inquiry was eagerly seized.

The Triassic and immediately overlying and underlying rocks of the North-west of England and the Midland Counties[3] were for the purposes of the Geological Survey classified by Professor Hull as follows:

Triassic Series.

- A1. Rhætic or Penarth beds occurring at Copt Heath and the South-west of England.
- A2. New Red Marl. Red and Grey shales, and marls sometimes micaceous, with beds of rock salt and gypsum containing *Estheria* and Foraminifera (Chellaston).
- A3. Lower Keuper Sandstone. Thinly laminated micaceous sandstones and marls (waterstones); passing downwards into white, brown or reddish sandstone, with a base of calcareous conglomerate or breccia.
- C1. Upper Mottled Sandstone. Soft bright-red and variegated sandstone (without pebbles).
- C2. Pebble Beds. Harder reddish-brown sandstone with quartzose pebbles, passing into conglomerate with a base of calcareous breccia.
- C3. Lower Mottled Sandstone. Soft bright-red and variegated sandstone (without pebbles).

[1] Presidential Address, Geological Section of British Association, 1886.

[2] On the Constitution and History of Grits and Sandstones, Q.J.G.S. 1881, p. 27.

[3] Triassic and Permian Rocks of the Midland Counties of England, Memoirs of the Geol. Survey, 1869, p. 10.

Permian Series.
1. Upper Permian. Red marls with thin-bedded fossiliferous limestones (Manchester).
2. Lower Permian
Red and variegated sandstone (Collyhurst, Manchester).
Reddish-brown and purple sandstones and marls with calcareous conglomerates and trappoid breccia (Central Counties).

Previous to Prof. Hull's investigations no subdivisions had been recognized in the Lower Trias or Bunter Sandstone, but that geologist, as the result of an extensive survey, considered that there were well-marked lithological differences in the beds which justified a classification into three horizons, namely, — 1 Lower Mottled Sandstone; 2 Pebble Beds; 3 Upper Mottled Sandstone. It is upon this scheme that the Geological Survey maps were and have since been constructed.

Considering the difficulties of the survey, much of the country being deeply drift-covered, the maps have fairly represented the geology of the areas surveyed, especially in those localities where the outcrop could be studied. It is in those places where the nature of the rocks could only be ascertained by boring that the weak points of a classification founded on lithological differences has naturally developed itself.

In view of the numerous borings for water supply and for other purposes since made in Lancashire and Cheshire, certain modifications have had to be made in the colouring of the maps showing the distribution of the subdivisions of the Bunter, and in some cases it is impossible to make the maps conform to nature and be logically consistent with the classification. Far be it from me to in any way seem to detract from the merits of the early attempts to conquer difficulties, especially where *any* classification is surrounded with pitfalls as it is in this case. It is a trying time for the geological surveyor when he is asked to predict the strata likely to be met with in a given bore-hole, yet this is what is demanded from a geological map; and if it occasionally errs, can we wonder?

Characteristics of the Triassic Sandstones.

For the purposes of this paper it will not, however, be necessary to discuss these refinements of subdivision. I propose to treat principally of the Bunter Sandstone and its mode of origin, and with this object in view it will be sufficient to look upon it as a massive agglomeration of siliceous sandstones of varying fineness or coarseness of grain, sometimes rounded, sometimes angular, more or less coloured by peroxide of iron, and often cemented together by secondary silica, or having crystalline growths thereon in optical continuity with the original quartz of the grain, even though the grain may have been previous to deposition well rounded by attrition of the surface. Within the interstices of the stone, and probably combined with the colouring matter, is a deposit of calcareous matter consisting of carbonate or sulphate of lime, sufficient, after continual circulation of water in the rock when brought about by artificial means, such as pumping, to impart a considerable amount of temporary and permanent hardness to the water.

If we add to these characteristics the occurrence of well-rounded pebbles of liver-coloured and of clear translucent quartzite, white vein quartz, and other hard rocks, from the size of walnuts to an occasional specimen having a longer diameter of four or five inches, we shall have exhausted our lithological description.

It is, however, to be specially noted that these pebbles occur often sparsely distributed through the rock, sometimes in nests, and in other cases in such numbers as to deserve the name of "conglomerate beds" given to them by Professor Hull.

So far as my experience goes, the liver-coloured quartzite pebbles are confined to the Bunter Sandstones; but I have not been able to satisfy myself so far that these pebbles occur at a definite horizon in the Bunter, except locally speaking. Indeed, I am much inclined to believe that in some cases the theory of the pebbles being confined to a central subdivision has led to a misinterpretation of these rocks.

Speaking broadly, there is a distinct lithological difference between the Keuper and the Bunter; for although in most cases it would not be possible to distinguish the rocks in local specimens, we cannot point to any rocks in the Bunter which would be mistaken by a practical man for Storeton or Grinshill building-stone. The transition from the Bunter to the Keuper is also more marked than between beds of the Bunter, nor is there in the Bunter any equivalent of the Waterstones of the New Red Marl.

The Bunter is evidently a sandstone deposit, while the Keuper, often beginning with a conglomerate bed, shades off into a lithologically distinct stone succeeded by thin-bedded sandstones, and finally by the great deposit of saliferous marls. These physical peculiarities are well known to geologists who have worked these rocks; but it is necessary for my argument to restate them in this connected form.

Theory of the Origin of the Triassic Rocks.

Hitherto I have said nothing of the topographical relations of the Triassic rocks; but as the distribution of the sandstones specially bears upon their origin, this cannot be neglected. Unfortunately of the arid sandy region of Central Asia, which it is suggested best explains the mode in which these Triassic Sandstones have accumulated, not much is known.[1]

If, however, the Bunter Sandstone is a riverine deposit, we would expect it to follow well-marked topographical features. Although there have been great orographic changes, some folding, and much faulting since the Triassic age, certain great features, such as the Pennine Chain, still remain in a modified form.

If we try in imagination to reconstruct a river, valley or valleys, or even a sandy plain, representing the deposits as they are conceived

[1] The following papers bearing upon the subject are well worthy of study: "On the Nature and Probable Origin of the Superficial Deposits in the Valleys and Deserts of Central Persia," W. T. Blanford, Q.J.G.S. 1873, pp. 493–502. "Alluvial and Lacustrine Deposits and Glacial Records of the Upper Indus Basin," F. Drew, *Ibid*, pp. 441–71.—"Journal Across Central Asia," by Lieut. Younghusband, describing Gravels from the Altai Mountains and Sandhills, 900 feet high, "Nature," 1888, May 17, pp. 65–6.

to have been laid down on the riverine hypothesis, we are met with numerous difficulties, which I for one find it impossible to solve. The Triassic Sandstones are found on both sides of the Pennine Chain; they occupy valleys in older formations such as the Vale of Clwyd, where they are flanked by Silurian hills, and in the Vale of Eden by the Carboniferous: and everywhere they appear to follow what seem to be the remains of the orographic contours of the ancient land. So far as my experience goes, there is a general tendency for the sandstones to thin off against these boundaries of more ancient rocks, even where these boundaries are represented by faults. No doubt the Triassic deposits formerly overlapped their present boundaries to a considerable extent; but there is a limit to this where they abut against high ground, for there is every reason to believe that the pre-Triassic rocks preserve in many respects their pre-Triassic orography.

My study of the origin of mountain-ranges has convinced me of the permanence of the more pronounced orographic features of a country, or what may be called lateral-pressure upheavals, which, while modified by faulting and denudation, can only after a lengthened period be finally destroyed by these combined agencies. From these considerations it is highly probable that the Triassic rocks which remain represent the deeper part of the basins in which they were laid down, and that they never encroached extensively on the Pennine Chain or the high land of North or South Wales.

In the case of more extensive and widespread deposits it is the thickest parts that, being upheaved into mountain ranges, have been most quickly destroyed by denudation. The Triassic rocks have not been affected in this manner, and therefore retain in a certain degree the basin-like form in which they were deposited.

One of the most remarkable features of the Triassic Sandstones is, speaking broadly, the persistency with which their lithological characters hold out over extensive areas, and their apparent independence of the bed-rock on which they lie, or that of the hills bounding the valleys containing them. Very few pebbles of the surrounding rocks are found in the Pebble-beds of the North-west of England, and it is mostly in the Midland Counties where any considerable proportion of local rocks occur in the conglomerate beds. On the other hand, the Permian beds underlying them are often largely made up of local rocks; instance the limestone breccia of Alberbury, composed almost wholly of fragments of Carboniferous Limestone, or the somewhat similar "Brockram" of the Vale of Eden.

Could we with any certainty localize the origin of the materials of the sandstones, a considerable step would be made towards unravelling this knotty problem; but even as regards the contained quartzite pebbles, one set of observers contend that they came from the north, another from the south, while a third considers that they have been derived from the destruction of rocks in Mid England.[1]

It would certainly seem, whatever be the direction from which

[1] See Bonney, Address to the Geological Section of British Association, 1886, and Geol. Mag. 1883, p. 199; 1888, p. 55.—Harrison, Proc. Birmingham Phil. Soc. 1882, vol. iii. p. 157.—Hull, Geol. Mag. 1883, p. 285.

the pebbles have travelled, that they are the product of other areas, and this will hold good, whether they resulted from the direct destruction of quartzite rock, or are of secondary derivation from a pre-existing conglomerate.

In framing a theory of the origin of the Bunter Sandstone, it appears to me that we cannot altogether detach it from a consideration of the underlying Permian.

Before these Permian deposits were laid down, and during the time of accumulation, extreme denudation of the Carboniferous rocks took place, and much of the material was derived from them; but on the incoming of the Trias, the materials became of a more uniform character, and were to a large extent of distant origin. It is not, however, improbable—nay, it is extremely likely—that the drainage or leaching of the Carboniferous rocks may have supplied the peroxide of iron with which the grains are coated, and which gives the sandstones their distinctive colour. We know also, for the records are unmistakable, that an enormous mass of Carboniferous sandstones and grits have been stripped from the Pennine Chain by denudation, and it is not unreasonable to ask what became of the resultant sand during Triassic times. The same may be said of the Old Red Sandstones of Herefordshire and the Quantocks. It would indeed seem incredible if these important formations did not yield their quota of sand to the Trias, though the absence of Carboniferous sandstone boulders in the Trias is remarkable. May they not have been ground to sand by currents which we know were capable of rounding the hard quartzite pebbles? It appears to me that neither the distribution, the uniform character, nor the great thickness of the Bunter Sandstone, accords with the subaerial river-delta theory.

No instance that I am aware of has been recorded of the finding in Triassic Sandstones of anything like a river-channel. It is difficult to conceive of a river unless fed from some very peculiarly constructed rocky area bringing down nothing but sand. The Nile does not do it; on the contrary, it covers the desert sands with fine mud. On the other hand, the subaerial building up of sandstones, proved by the boring at Bootle to be over 1200 feet thick, does not seem to be, considering what has been preserved of the orographic features of the time, a very plausible supposition.

While considering with many other geologists that the lacustrine theory of the origin of the Bunter Sandstones may be dismissed as altogether inadequate to account for the great prevalence of current bedding, the presence of well-rounded quartzite and other pebbles, and the absence of marl except in occasional thin beds, the substituted riverine theory seems to me to fail for the reasons already given. So far I have indulged principally in destructive criticism, proverbially the easiest sort of work, but not on that account less necessary in an attempt to arrive at truth. I have, however, elsewhere already indicated an alternative theory,[1] which I submit in more detail for the consideration of my brother geologists.

[1] Physiography of the Triassic Period, "Naturalist," pp. 108-111, April, 1889.

If, as I have reason to believe, the Triassic deposits of the North-west of England as they now exist fill up the sites of pre-Triassic basins and valleys, the absence of underlying Permian rocks over a large area surrounding Liverpool is a remarkable fact. The Bunter Sandstone was proved to be at least 1200 feet thick in the Bootle boring, and the additional 200 feet of sandstones and marls below that depth did not present any decided Permian characteristics. They became gradually more calcareous and of a deeper purple colour.

It is on the margin of the Triassic deposits that the undoubted Permians occur, and these facts would seem to point towards an unconformity between the Trias and the Permian. If this be a legitimate deduction, the Permian rocks must have been largely removed by denudation from these valleys before the Trias was laid down.

Whence then came the physical change which turned the valleys into areas of deposit? If they were filled up in the way the sub-aerial delta theory requires, we shall have to postulate the existence of very high land, probably of a granitic nature, for a river could not pile up a homogeneous delta of sand and pebbles from 1200 to 2000 feet thick without having a considerable gradient. If then such a plateau or Alpine range then existed, what has become of it? Why should it have been destroyed, when a lesser orographic feature like the Pennine Chain remains?

If, however, we look upon the areas in Lancashire and Cheshire occupied by the Triassic rocks as *subsided valleys* forming arms of the sea in Triassic times, and we can show with a reasonable degree of probability that sandstones having the characteristics of the Bunter could be laid down under such conditions, we shall have advanced a considerable step towards a solution of the problem.

I have shown that tidal action affects the bottom of the sea to the profoundest depths,[1] and is especially effective in embayments and straits. The Trias of Cheshire and partly that of Lancashire lies in an embayment between the Carboniferous mountains of Wales and the Carboniferous hills of Lancashire and Cheshire, and this is connected with the Birmingham Triassic Basin and the strait-like neck of Trias at Bridgenorth.

Indeed, if a subsidence of England and Wales were to take place now to the extent of 400 feet, most of the Triassic deposits would be submerged, and the embayment, though more extensive, would apparently follow or be concentric with the Triassic boundaries. This has long appeared to me a remarkable fact, and a strong testimony of the permanence of certain orographic features of the earth's surface. In this connection we must not lose sight of the possibility of a great extension of the Triassic Sandstones existing under the bed of the Irish Sea. Of this there is the strongest probability from the known existence of bordering deposits of Trias on the coasts of England, Scotland, and Ireland, sending offshoots up the valleys in the way already described.

[1] Tidal Action as a Geological Cause, Proceed. of L'pool. Geol. Soc. 1873–4; Tidal Action as an Agent of Geological Change, Phil. Mag. May, 1888.

But where have the bulk of the materials travelled from? If we look to the geological structure of the lands to the north, there do not appear to exist rocks in sufficient quantity of a nature to supply the great mass of quartzose sand constituting so large a bulk of the Trias. The English Channel, judging from the islands dotted about it and the peninsula of Cornwall, appears to be a granitic and gneissic area, and it is possible that much of the Triassic Sands may have travelled from this locality. To bring this material through subaerial denudation within the distributing grasp of the tides a regional elevation of a thousand feet would be largely effective. It would become a land area supplying quartz-grains to build up the sandstones and decomposed felspar as a contribution to the marls.

From the indestructibility of the quartz-grains they would last longer and travel farther than the other mineral constituents of the rock. Concurrently with this elevation the land occupied now by the Triassic deposits must have been relatively depressed, giving entrance to the sea in the way already sketched out.

It may possibly be urged in objection to this view that there is no connection between the Lower Trias of the South-west of England and that of the Midlands. We know, however, that there is as regards the Upper Trias along the Severn Valley, and it is quite likely that to the east of this line the Lower Trias may underlie the Lias[1] and Oolites. The lie or disposition of the overlying formations favours this idea, and there are no borings to disprove it. Any one who has practically attempted to prove continuity by borings over even a limited area knows how uncertain the method is.

It is also not impossible that the anticlinal ridge—not necessarily an impassable barrier of Palæozoic rocks—connecting the Mendips in Somersetshire with the Belgian Coal-fields, so sagaciously pointed out by the late Mr. Godwin-Austen, may have yielded contributions to the Trias, for the existence of Old Red Sandstone in this ridge under London has been proved.

May not some of the quartzite pebbles have had their origin here also? for, as already stated, they diminish in size and number as we go northwards. Even at Market Drayton in Shropshire the pebbles in the conglomerate are more numerous and larger than in Cheshire or Lancashire. Pebbles of quartzite containing fossils similar to those of Budleigh Salterton are reported by Mr. Jerome Harrison and others as occurring in the Trias of the Midlands and in the drift derived therefrom. The boulders and pebbles of Budleigh Salterton also appear to be larger on the average than those of the Midlands, which facts seem to point to a common derivation and a longer travel of those of the Midlands.[2]

[1] See records of some remarkable borings in Woodward's "Geology of England and Wales," second edition, facing p. 612, also the accompanying Geological Map of England and Wales.

[2] See Quartzite Pebbles of the Drift and Triassic Strata of England, Proc. of Birmingham Phil. Society, vol. iii. p. 157 (1882); On the Triassic Rocks of Somerset and Devon, W. A. E. Ussher, Q.J.G.S. vol. xxxii. p. 367 (1876); Notes on the Classification of the Triassic Beds of the South-West of England, H. B. Woodward Geol. Survey Memoir on E. Somerset and Bristol Coalfields; Red Rock Series of the Devon Coast, Rev. A. Irving, Q.J.G.S. 1888, p. 149.

Then again, as before stated, although Carboniferous Sandstone boulders are scarce in the Trias of the North-west of England, we cannot reasonably ignore the fact that massive sandstones and grits exist in the Pennine Chain, which have been subjected to vast denudation. Considering that the strong current bedding indicative of turbulent water action is one of the characteristics of the Triassic Sandstones, and one of its difficulties, may we not reasonably assume that Sandstone boulders would soon get ground to sand? There is very little of the marginal boundary of the Trias left in the North-west of England, and it is in this fringe that Sandstone boulders would most likely occur. Marginal deposits of local rocks seem to have been preserved in Somerset and Devon.[1]

Given these materials, tidal action, as I have proved in the papers referred to, is quite capable of selecting, distributing, and accumulating them in the form in which they appear in the Triassic deposits.

On this theory we are not limited to depth, *as the tidal wave produces currents acting through the full depth of the water from the surface to the bottom*, and current-bedded sandstone might be laid down by this agent at a depth of many hundred feet and over very extensive areas.

These ideas relating to the physiography of the Triassic period have been placed before the Geological Section of the British Association simply as suggestions for their consideration. The subject is beset with difficulties, but in solving such a problem all possible agencies must be tried and discussed.

A new hypothesis also has this merit, that it guides men's thoughts into fresh channels, and it is only by observations renewed again and again on various theoretical lines that the truth can be at last reached. There is, doubtless, one objection that will be urged against the theory here set forth, namely, that it does not account for the entire absence of fossils in the Bunter Sandstone, while the subaerial river-delta theory does.

This, doubtless, is one of the many difficulties of the question; but on the other hand if marine fossils had been found, the difficulty of interpretation would not have existed, nor would it had the sandstone contained fresh-water remains. This argument, although possessing considerable force, is only one of negative evidence proverbially unsafe. Also, if we admit marine action, it does not exclude that of the winds. Æolian deposits would be certain to be extensively developed on the margin of such sand-laden waters, and thus sandstones, which do not contain pebbles, have much rounded grains and are irregularly current-bedded may be due to wind-action.

As regards the Keuper Marls, the dessicated lake theory fits in well with their characteristics, and it may be that the Keuper Sandstones show the gradual passage from marine to fresh-water conditions. The upper beds of the Keuper Sandstones show well-developed ripple-marks which are not common, but are not absolutely absent in the Bunter. The replacement of massive by thin sandstone beds, and the intercalation of shales, together with the

[1] See papers already quoted.

presence of pseudomorphs of chloride of sodium, show that a new condition of things set in with the Keuper. In conclusion, I may point out while on negative evidence that the absence of saline deposits in the Bunter is on the other hand against the subaerial river-delta hypothesis, as the sandy deposits to which they are compared often eventually absorb the rivers which create them; and this applies to the North of Africa as well as to the Asiatic example.

NOTICES OF MEMOIRS.

THE GEOLOGY OF DEVON, FACTS AND INFERENCES, FROM THE PRESIDENTIAL ADDRESS TO THE DEVONSHIRE ASSOCIATION. By W. H. HUDLESTON, Esq., F.R.S., Sec.G.S., etc. August, 1889.

Part II.

(*Concluded from November Number*, p. 514.)

TAVISTOCK COUNTRY.

THE geological phenomena in the neighbourhood of Tavistock are of such interest that I cannot do better than close this address with a brief allusion to some of the features of the west side of Dartmoor, and the adjacent country.

It is a region which has always presented peculiar difficulties, but the new line of railway in course of construction may help to clear matters up. The subject can be grouped under four headings: (1) The structure of the country; (2) the nature of the basic igneous rocks, or "greenstones"; (3) Dartmoor; (4) the metalliferous deposits.

(1) *The structure of the country* on the west side of Dartmoor differs considerably from that on the east side, more especially in the fact that the Devonian beds are represented as dipping towards the sea instead of away from it; at least this is the case for several miles immediately north of Plymouth. Further towards the north, in the direction of Tavistock and beyond, there would seem to be a complexity of structure unusual even for Devonshire. Consequently the boundary between the Devonian and Carboniferous, as laid down in the Survey Map, may be subject to considerable revision. Mr. Worth astonished us lately at the Geological Society by the statement that the town of Tavistock is actually on the Carboniferous, and yet that, owing to a complex series of foldings, the Devonian rocks are brought up on both sides. I know of no spot in the United Kingdom where the geological boundary-lines seem to be so much under discussion at the present moment.

Although Mr. H. B. Woodward, in the map attached to the "Geology of England and Wales," follows De la Beche in assigning the Brent Tor district to the Carboniferous, it has long been claimed as Devonian by some geologists. These views are perhaps the result of Mr. Rutley's interesting work on the schistose volcanic rocks west of Dartmoor, described as consisting of alternations of lava-flows, tuffs, and tufaceous sediments. That this class of rock, locally

known in its vesicular form as "honeycomb dunstone," was of a volcanic nature, had long ago been recognized by De la Beche; but it was the late Mr. John Arthur Phillips who first clearly demonstrated, in his classical papers on the Cornish "greenstones," that many of these beds were actually lava-flows. Mr. Rutley went a step further, and considered that he had found in Brent Tor a fragment of one of the old volcanic necks. His famous diagram, with its column of ashes flattened by the wind, described by himself as as "a chimæra which may embody a certain amount of truth," is familiar to all geologists. Nay so graphic was the picture, and so convincing the arguments, that a certain Mr. Thorpe fancied that he had corroborative evidence of the prevalence of the south-west wind in Devonian times, because, forsooth, he had found lapilli from Brent Tor in the joints of a limestone at Newton Abbot.

Let us express a hope that before the Association next meets at Tavistock the boundaries between the Devonian and Carboniferous may have been made as clear as noonday, and accurately laid down on a six-inch map, which shall itself be a model of chartography.

(2) We must now take into consideration the *nature of the basic igneous rocks*, commencing with those which are interbedded, most of which are now said to be of Devonian age. Before doing so it will be necessary to say a few words about the "killas," a very loose term better understood by miners than geologists. Judging from Mr. Worth's remarks on the stratigraphical relations of the Devonian rocks of South Devon, most of the "killas" of this district belongs to the grey and drab slates intersected by lodes and elvans, which was described by Conybeare as the metalliferous series: above this comes a group more variable in its nature, which is especially characterized by interbedded volcanic rocks, and Mr. Worth suggests that the Brent Tor series may belong to this group: above these again are the purple and green slates immediately underlying the Plymouth limestone. The main point to notice is, that the whole of this slaty series is regarded as below the Plymouth limestone. Consequently it must belong to the lower part of the Middle Devonian, and possibly to even lower beds. Mr. Rutley, if I recollect rightly, regarded the Brent Tor series as possibly in the Upper Devonian.

The interbedded basic igneous rocks, then, are placed by Mr. Worth a long way below the Plymouth limestone; whereas the late Mr. Champernowne, in an interesting posthumous communication to the Geological Society, was disposed to regard his Ashprington volcanic series as above the main limestone of that district. In reference to this difference of opinion two points seem to present themselves for consideration. Firstly, that the schalsteins need not be confined to any particular horizon in so thick a series as the Middle Devonian; secondly, that the phenomena of extravasation, whether interbedded or transgressive, is limited, with very unimportant exceptions, to the southern portion of the county, from whence the line of igneous products may be traced into Cornwall. Hence the area of erupted rock is local, and to a certain extent

linear, and is probably not absolutely confined to any particular geological horizon.

The interbedded basic igneous rocks have been described by numerous authors, and their general petrographic features are fairly well known. In Northern Cornwall, according to Mr. J. A. Phillips, these ancient lavas are called "dunstones." Specimens analyzed by him were found to contain 42 per cent. of silica, over 20 per cent. of alumina, and the alkali is almost entirely soda: the amount of lime is nearly twice that of magnesia, and there is over 12 per cent. of protoxide of iron. From a chemical point of view these rocks, then, are allied to the basalts.

The intrusive "greenstones" are classed by Mr. Worth under three heads. They are sporadically developed, but seem to be most numerous and of the largest size in the vicinity of Dartmoor: they are believed to be older than the Dartmoor granite, which is said to alter them. If the Survey mapping is correct, the so-called gabbros between Marytavy and Wapsworthy occur in Carboniferous rocks, and must of course be younger than the beds into which they are intruded. These gabbros, Mr. Worth considers, are the vestiges of a widespread pre-Dartmoor igneous activity, producing basic rocks. He points out that their relations to the granite, both here and in Cornwall, are too persistent to be accidental, and he suggests that they may represent the basic forerunners of the more acidic granites. The age of the rocks into which the Marytavy "gabbros" have been injected still remains to be settled, but the notion that either they or the granites have brought up the lowest stratified rocks is not borne out by experience in other parts of the area round Dartmoor.

(3) Having now cleared the way a little by a brief glance at the containing rocks, we are in a position to attempt the study of *Dartmoor* itself, that supreme monument of the old eruptive forces. Dartmoor, as every one knows, is contained partly in Devonian and partly in Carboniferous rocks, and from the position of the *Posidonomya*-beds it is probable that the lower part of the Carboniferous adjoins the granite. Mr. Ussher, speaking of the beds on the northern and eastern flanks of Dartmoor, observes that the Culm-rocks dip off the granite above Belstone in a marked manner. He also says that the Culm-rocks on the north are roughly parallel in their strike to the margin of the granite, whilst on the east and west their strike is cut off, so to speak, by the granite or else deflected. These considerations are of importance as showing how the granite lies in its case.

From what has already been said, it is perfectly clear that this granite is in nowise connected with anything of the nature of an anticlinal axis bringing up older rocks. In fact on the east side, where it abruptly terminates, its relations to the adjacent country are almost those of a synclinal. On the Tavistock side its relations with the adjacent country are more obscure, owing to the stratigraphy of the district being as yet undetermined. Moreover, there is probably underground connection on this side, through Hingston Down, with the granite boss of Brown Willy.

The above considerations tend to show that the relations of the granite to the surrounding rocks are somewhat peculiar, and that it is not exactly easy to frame a theory to satisfy all the conditions. The composition, which is that of a normal potash granite, and the contact phenomena, are clearly against the notion of any large absorption of the containing rocks, such as are now accessible to observation. There has been much nonsense talked about granites being the result of the extreme metamorphism of the beds in which they occur. Mr. J. A. Phillips in his paper "On the Rocks of Cornwall in relation to Metalliferous Deposits"[1] showed very clearly that, although the different kind of killas vary materially in composition, under no circumstances could the mere re-arrangement of the constituents result in the production of granite.

He gives a table with the chemical compositions of ten varieties of Cornish killas, showing a range in silica from 33 to 68 per cent. and of alumina from 10 to 24 per cent. The alkali is mainly soda, and of this there is a considerable amount in some specimens, pointing to the conclusion that killas has been largely derived from the dissemination of very fine volcanic matter of a basic composition. This coincides with the prevalence of contemporaneous volcanic phenomena. It is worth noting that the roofing-slate of Delabole affords an exception to this rule, in containing more potash than soda.

As there is no reason to suppose that the early chemical history of the Dartmoor granite differs materially from that of the Cornish granites, their sources must have been deep-seated, and they must have originated under the ordinary conditions which produce the granitic magma, whatever those may be. The main questions remaining to be considered are the period and circumstances under which the Dartmoor granite assumed its present position.

There is no evidence at present, as far as I know, which would enable us to fix the period any nearer than the somewhat vague date "the close of the Carboniferous." Dr. Barrois says that many of the Brittany granites are of Carboniferous age. But in the case of Dartmoor it is probable that the great foldings of the Hercynian mountain-system had been mainly effected, and the synclinal of Devonshire formed, before the granite was insinuated. In position the mass of Dartmoor is outside the axis of the Cornish granite; if their alignment was followed the centre of Dartmoor would be about Hatherleigh. Hence the stratigraphical position differs somewhat from that of the Cornish granites, although possibly their age may be quite the same. It is enough to know that an immense physical revolution was effected all over the British Isles between the close of the Carboniferous and the beginning of the Permian, and the intrusion of the Devon-Cornwall granites must have taken place either then or in early "red rock" times.

Next, as to the circumstances under which the Dartmoor granite found its way into its present position. Last year Mr. Ussher treated

[1] Quart. Journ. Geol. Soc. vol. xxxi. p. 319.

this great question with characteristic ingenuity, and showed pretty conclusively that neither the punching theory nor the absorption theory would meet the facts of the case. De la Beche gave us a valuable hint, as indeed he was in the habit of doing, when he inferred that, owing to the volcanic activities which had prevailed in the area during the deposition of the Palæozoic series, a line of least resistance to a body of granite, impelled upwards, might have been formed. In this way the granite of the great bosses may have been forced through ground already weakened as the site of old volcanic vents—such as Brent Tor, we might add.

Of course, it must be remembered that the contacts we now see only represent a certain stage in the relations between the granite and its case. A million years ago, when the country was much higher relatively, the contacts may have presented a somewhat different phase, whilst it is certain that those who are able to inspect the contacts after another million years of atmospheric denudation, will at least get much nearer to the roots of the matter. As far as I am able to judge from Mr. Ussher's descriptions, there are indications of a considerable lateral thrust on the north and on the south side of the mass, parallel to the mean strike of the enclosing beds. This looks very much as if the main displacements which took place were lateral, the beds yielding to the pressure gradually, and thus helping to intensify the flexing of the district.

How far the evidence is in favour of Mr. Ussher's suggestion that Dartmoor is a laccolite, insinuated at the junction of Devonian and Carboniferous rocks, I am unable to say. This seems a somewhat ignominious termination to a career which patriotic Devonians have regarded as nothing less than the plutonic supply-pipe of a regular volcanic cone, more lofty than that of Etna. Possibly the two theories may be reconciled by regarding the supposed laccolite as a kind of reservoir, or local thickening in the pipe.

It was Professor Bonney who first set the Devonshire geologists on the look-out for the vestiges of the great Devonshire volcano. Not a mere Brent Tor this time, erupting its lavas into the Devonian Sea, but one of a line of lofty peaks of far later date. "Among the many excellent geologists and enthusiastic students of the West of England," said he, "is there no one who will undertake to replace the covering which has been stripped from the granitic bosses?" He also indicated that a thorough study of the "red rocks" of Devonshire would yield important results in this direction.

Mr. Worth is amongst those who have responded to this challenge, a circumstance to which allusion has already been made in dealing with the New Red question. It is somewhat singular that if there really was a volcanic cone covering the Dartmoor pipe, the traces of it should have to be sought at the eleventh hour in the "red rock" breccias. These ought to be full of unmistakeable fragments of old acidic lavas, and of the felsites which are structurally intermediate between such lavas and granite. Possibly the want of adequate petrographic knowledge may have hitherto retarded the discovery, and we naturally await the result of further investigations.

But Mr. Worth himself has supplied evidence which goes far to explain the presence of remnants of felsitic, and even of volcanic rocks, in accumulations more recent than the "red rock" breccias. Such remnants are much more likely to have been derived from the elvans, which form so characteristic a feature in the country between the Dartmoor and Brown Willy granites, and some of which probably reached the surface in a more glassy condition than the portions now accessible to operations. Besides, even the existing dykes are represented in some cases as developing a semi-vitreous ground-mass with porphyritically imbedded crystals.

Theoretically it is extremely probable that the granite bosses of Devon and Cornwall may have passed upwards into volcanic rocks, and that consequently they represent a line of eruptive vents which were possibly active in Permian times, or those immediately preceding. But the petrological evidence alone is not conclusive. If we suppose that the "red rock" breccias are of Triassic and not of Permian age, all, or nearly all, traces of the volcanoes might have been removed before the breccias were accumulated. Clearly the granite, with its characteristic crystals of orthoclase, had been laid bare when the beds containing Murchisonite were deposited.

As regards the composition of the Dartmoor granite, the accessory minerals such as schorl, and the proneness of portions to kaolinization, are especially noteworthy. This latter feature has a tendency to produce unequal weathering, and it is not at all improbable that the Tors are in a great measure due to the unequal weathering brought about by this cause. They represent portions which, in the hour of trial, were harder and perhaps chemically more stable, and consequently less liable to disintegration. The forms of the Tors, as was pointed out by Prof. Rupert Jones and more recently by Mr. Ussher, have been largely determined by the arrangement of divisional planes, the mass being intersected by what the latter calls impersistent cracks, running more or less horizontally and crossed vertically or obliquely by joints. Variation in the direction of these joints is accountable for much of the variety in the Tors themselves.

(4.) *The Metalliferous Deposits.*—The abundance of schorl, especially on the edges of the granite, and the kaolinization of the felspars, are indirectly connected with the last subject which it is proposed to bring to your notice; viz. the origin of the metalliferous deposits for which this region is so famous.

It is, I believe, admitted that the great east-and-west fissures through which the elvanite has been injected were formed after the consolidation of the main mass of the granite, though their chemical composition points to their having been derived from the same magma as the granite. The next step in this curious underground history appears to have been the formation of a series of empty fissures, most of them having a more or less east-and-west orientation. And now commenced a fresh set of phenomena which, in an extremely modified sense, may be said to be still in operation.

The fissuring of this region was probably due to reaction after the strain consequent on the system of folding, to which allusion

has been so often made. When first this fissuring, or gaping of the rocks, occurred, there was a supply of molten silicates from below more than sufficient to fill up the void. But, as often happens in volcanic regions of modern date, the last stage of primary activity is represented by fissuring without injection of molten matter. A number of open cracks are thus formed, which favour the circulation of underground waters, often intensely heated, and not seldom passing off as condensed steam where they happen to reach the surface.

In Devonshire and Cornwall, the period when this phase was at its height occurred most likely in late Permian and early Triassic times.[1] But, as every one knows, there have been many periods of shifting amongst the rocks; and doubtless the country must have participated in the great Tertiary earth-creep which folded the Downs and the Isle of Wight, about the same time that the Alps were being raised into a mountain-chain. Each successive movement would be apt to produce modifications in the underground circulation, cramping it here and stimulating it there; and doubtless, as the temperature decreased, the solvent powers of the waters would diminish also.

To such underground circulation in old volcanic districts like this, most of the phenomena in connection with metalliferous veins are due, though it must always be remembered that here we see a plutonic phase of what were volcanic activities at higher levels in earlier times. Fifty years ago De la Beche and the first Surveyors evinced an intense interest in this subject, and in the Memoir already referred to many hypotheses of origin are discussed. Since those days the world has been revolutionized in more ways than one, and in no way more than in the transfer of mining enterprise. But the experiences of the last five-and-twenty years in the Tertiary volcanic districts of North America have not been lost upon the numerous able men who have been employed as engineers or surveyors in those highly metalliferous regions. The late John Arthur Phillips left a record of his great knowledge and experience in his excellent treatise on *Ore Deposits*. And I have no doubt that many here are more or less acquainted with the important works of the French *savant* Daubrée, whose "*Etudes synthétiques de Géologie expérimentale*," and "*Les eaux souterraines aux époques anciennes*," furnish us with an immense amount of information on the origin of metalliferous veins.

Briefly, it may be said that the underground circulation theory is the one most generally adopted, the chief difference of opinion being as to the relative importance to be assigned to *lateral secretion* and to *ascension* respectively; or, stated in simpler terms, whether

[1] Since the publication of portions of the address in the local papers, Mr. Thomas Collins, of Redruth, has written to say, "he considers there is evidence in the Tavistock district that the metalliferous deposits containing copper-ores had been formed in the Devonian rocks before the deposition of the main mass of the Carboniferous. The large copper-lodes of Mary Tavy, for instance, are in Devonian rocks, and cease altogether on coming into contact with the black schists of the Carboniferous." It is suggested that this may be due to faulting at the junction.

the vein-material comes from the sides or from below. It is reasonable to suppose that both sources may have contributed to the supply, though in certain cases a change in the deposits, accompanying a change in the country rocks, would seem rather to favour the notion of lateral secretion. Thus Mr. Phillips remarks, with regard to the Tavistock district, that the copper-ores are often associated with a blue clay-slate. If the slate becomes deeper in colour, iron-pyrites alone occurs; and if the rock becomes quartzose, even the pyrites disappear.

Whilst endeavouring to trace the source of the ores in metalliferous lodes we should bear in mind the experiments of Sandberger, who found that the heavy metals occur in the silicates of the crystalline rocks of every age. Augite and the magnesia-micas are especially rich, and the lithia-micas are noted as being stanniferous. The origin of tin-ores is probably different to that of the sulphuretted ores, though both are often best developed at the junction of igneous and sedimentary masses. This, of course, is partly accounted for by greater facilities for fissuring, and still more by an increase of heat, which is likely to promote the underground circulation, and above all to increase the solvent power of underground water.

The question of solution has always been a difficult one, and has inclined some people to adopt the notion of sublimation of the metallic sulphides. As an alternative theory we have had the reduction of sulphates by organic agency. But people are beginning to think that both these agencies may be dispensed with, and that, under peculiar conditions of heat, pressure, and dissolved gases and salts, the solvent powers of water may be largely increased. Anyhow, it is perfectly certain that metallic sulphides, such as cinnabar and pyrites, are being deposited from hot springs along with various forms of silica, both in California, and at Steamboat Springs in the State of Nevada. We may well believe that this latter place, of which an account was given in the Quarterly Journal of the Geological Society as long ago as 1864, represents with a certain amount of fidelity the conditions which prevailed in the upper portions of the metalliferous lodes of Devon and Cornwall during a period, not of maximum activity, but when a considerable deposit was taking place.

The solution and transport of tin-ores are capable of a different explanation. As is well known stanniferous deposits are not only very local, but are also accompanied by a peculiar group of minerals, such as topaz, schorl, axinite, and fluor, which contain a notable quantity of either Fluorine or Boron, and in the case of schorl of both these elements. Daubrée observed that this is the case wherever tin-ore has been found, and he suggested that, in the first instance, tin was brought up from what he calls the general reservoir of the heavy metals as a fluoride. The interesting chemical experiments connected with this ingenious hypothesis are detailed in his great work on Experimental Geology. According to these views stannic-fluoride and steam would decompose each

other at a moderately high temperature, the result being a deposit of binoxide of tin or cassiterite. The liberated hydrofluoric acid, besides helping to form such minerals as schorl and other fluo-silicates and fluorides, would enter into the general circulation of the rocks, and thus tend to facilitate that kaolinization of the felspars which has produced so much china-clay on the south-west side of Dartmoor, and in the mass of the Hensbarrow granite.

It is also worth noting in this connection that, according to Dr. Le Neve Foster, the great flat lode of Carn Brea, near Redruth, is in the main a band of altered rock, and he is inclined to suspect that half the tin-ore in Cornwall is obtained from tabular masses of altered granite. In such cases there is no regular lode, but very fine cracks in the rock have evidently given access to stanniferous solutions, which have deposited oxide of tin more or less abundantly in the vicinity of such cracks, and materially changed the nature of the original granite.

The phenomena in connection with these impregnations of tin-ore appear to favour Daubrée's views; but such points are to be commended to the notice of local geologists, who alone can test their suitability to explain the facts which come before them. I would merely remark that too much stress should not be laid on such cases as those of deer's antlers having been found partly replaced by cassiterite in the old river-gravels. This has been effected at ordinary temperatures, most probably by the aid of alkaline carbonates arising from the atmospheric decomposition of felspars, and proves that the most insoluble minerals may be successfully attacked by agencies now or lately in operation, and their metallic element moved from point to point, but only in very small quantities.

It is to be feared that chemical questions such as these possess but little interest for the members of the Association, and I apologize for having introduced them, however briefly, before a general audience. But there are certain conclusions which we are able to draw without any special reference to chemistry. In the great metalliferous lodes we see the roots of old mineral springs and geysers, which spouted their water and steam into the air, and perhaps covered the surface of the ground with siliceous sinter. That was a time when the volcanic forces of this remarkable region were on the wane, and after the great outpourings of lava had taken place upon a surface of which every trace, perhaps, has been swept away. How long these hydrothermal agencies continued to be active we cannot tell; but it is by no means improbable that they were in operation throughout a considerable part of Mesozoic time, during which period the spoils of this western land, brought down by the ceaseless forces of denudation, partly found their way into the eastern sea, and thus helped to build up the deposits which were afterwards to be fashioned into the Secondary rocks of England.

REVIEWS.

I.—Chemical and Physical Studies in the Metamorphism of Rocks, based on the Thesis written for the D.Sc. Degree in the University of London, 1888. By the Rev. A. Irving, D.Sc. Lond., Senior Science Master at Wellington College. 8vo. pp. 138. Price 5*s*. [August 16, 1889.]

THE word "Metamorphism" as applied to rocks, in consequence of the various interpretations to which it has been subjected, is no doubt of great service to slippery disputants. By a judicious use of the term some writers can conjure up such a haze as most effectually to elude the pursuit of any critic who rashly endeavours to run them down. Dr. Irving wishes to put an end to this state of things by fixing such limitations to the meaning of the term as would, for instance, prevent people from quoting examples of an early stage of "metamorphism" from such rocks as the Old Red Sandstone Conglomerates of Scotland. In this thesis "metamorphism" is to be used to mean only changes in the internal structure of rock-masses. The author, however, qualifies this by subsequently stating that he is not dealing with the so-called rocks of the systematist, but rather with *principles*. Hence, he is disposed to ignore the restrictions and limitations which may be convenient in classifying rocks; and, as to text-books, they appear to arouse in his breast feelings akin to those of the Knight of La Mancha at the sight of a windmill.

In his general conclusion as to the diagenetic origin of the so-called "Archæan" rocks we are disposed to concur, although it is by no means improbable that many of these rocks may have had a very different and much later origin. But it is by no means certain that the arguments and peculiar nomenclature of the present work will be found, in the sequel, to have strengthened the diagenetic as opposed to the metamorphic theory. No doubt those who can thoroughly understand the book may derive considerable benefit from its perusal. At the same time, the chemical speculations of of this author, as to the earlier stages of rock-genesis, appear by no means equal in quality to those of Sterry Hunt, though this perhaps may be regarded as a matter of fancy, and other readers might arrive at a different conclusion. There is a considerable parade of chemical knowledge brought to bear upon these points.

The plan of the treatise is as follows:—

Paramorphism, or Mineral Change, includes Primary Paramorphism or Rock Genesis, and Secondary Paramorphism resulting from the gradual alteration of the conditions of rock environment. *Metatrophy* includes changes in the physical character of the rock-masses, whilst there is no essential change either in the rock-mass or in its constituents. *Metataxis* is a change of order of the constituents of the rock-mass of which the phenomena of slaty cleavage may be taken as a typical instance. The author then proceeds to deal with what he calls *Hyperpheric* Change, of which the dolomitization of

limestones may be taken as an example. There is a section on Contact Metamorphism, which, together with general remarks on Metamorphism, and two Appendices, completes the work.

Dr. Irving is a professed disciple of Credner, and has made free use of his writings, as well as of the German literature of the subject generally, nor has he been forgetful of the writings of English petrographers in the construction of his treatise. In addition to this he combines his own considerable experiences as a geologist in the field and as a chemist in the laboratory. There can be no doubt, therefore, that much interesting and valuable matter is contained in these pages. Moreover, few will disagree with the statement in the preface that the truest teaching is that which stimulates the mind to active thought, not that which saves the student the trouble of seeking by loading the memory with second-hand knowledge. All this is excellent, but when he talks about a tendency to fetter the discussion of scientific questions by a spurious orthodoxy, Dr. Irving is again tilting at a windmill. The two schools which respectively put their trust in diagenesis and epigenesis enjoy the most perfect freedom of discussion at the Geological Society, and that they make ample use of it any one who is in the habit of attending the meetings can testify.

II.—The Geology of London and of Part of the Thames Valley. By William Whitaker, B.A., F.R.S. Vol. I. Descriptive Geology, pp. xii. 556, Folding Table. Price 6*s*. Vol. II. Appendices (Well-sections, etc.), pp. iv. 352. Price 5*s*. Geological Survey Memoir. 8vo. (London, 1889.)

NEVER before has the geology of any tract of country been described in such detail. We say this advisedly, bearing in mind other publications (the result of private or of official enterprise), whose object has been to describe as fully as possible the geology of particular areas. There is good reason why London should yield such ample information, and it is fortunate in having so enthusiastic and so careful an exponent of its geological record.

In the country around London there have been more numerous exposures of the strata than elsewhere, in the foundations for houses, not to mention the excavations for gravel, clay, and chalk, and the railway-cuttings. Moreover, not only has the surface structure been revealed in so many places, but the underground geology has been proved in the numerous wells and borings, several of the latter being very deep, and furnishing information of high geological interest. Hence no other part of the world could have contributed such a mass of geological facts.

To begin at the end of the work, we may mention that the records of well-sections (given in Vol. II.) number nearly 800; and in addition there are notes of numerous trial-holes and many other sections. This volume in itself will be of great practical value to well-sinkers and other engineers; but notwithstanding the mass of information he has so carefully arranged and tabulated, with all authorities indicated, the author still craves for more—asking for

further information concerning any of the wells, and for records of any new sections.

Turning now to the first volume, we may note that the whole of Middlesex, and parts of Oxfordshire, Bucks, Herts, Essex, Berks, Surrey and Kent, are included in the country described. It is indeed the tract represented on Sheets 1, 2, and 7 (and northern portions of 6 and 8) of the Geological Survey Map. So far as regards the Chalk and Eocene strata (the description of which occupies about one-half of the volume), the work may be regarded as, in great measure, a new edition of Mr. Whitaker's Memoir on the Geology of the London Basin (Memoirs Geol. Survey, vol. iv.), although some portions of the area described in that Memoir are outside the limits assigned to the work before us.

The parts relating to the Glacial Drifts and newer deposits, filling the second half of the volume, are for the most part new, including as they do the description of the beds in the southern portion of Essex. In short, about two-thirds of the entire work may be considered as new.

A record of facts, valuable enough in questions of practical geology and as material from which scientific conclusions may be drawn, cannot of course furnish matter which even the most enthusiastic student would care to read steadily through. Details, however, are printed in small type, while general remarks on the more important facts, historical reviews, and conclusions are printed in larger type, These latter will be read with advantage and interest. The author indeed has so exhaustively studied the literature of the subject, that the reader, unless he be anxious to learn in more detail the theoretical views of other workers, need scarcely refer to the previous literature, for Mr. Whitaker has acknowledged all sources of information. It need hardly be said that the author takes an eminently practical and common-sense view on debateable subjects, and while his remarks are written in a judicial spirit, they are at the same time cheery and not seldom humorous.

Students of Mr. Whitaker's writings are aware that the author has not manifested much liking for purely theoretical or speculative geology, at any rate he has hitherto abstained more than many men with much less experience from expressing theoretical views. But now we are glad to find the author, in dealing with the River Drift, makes the following remarks (p. 329): "If, however, in the Historical Review of the subject, objection is often taken to the opinions quoted, and my own view is sometimes given in a by no means undecided manner, let it not be put down to the absolute loss of former modesty (and modesty should grow with knowledge); but to the fact that one is bound, in the present case, to state one's own opinion, derived from prolonged study of the beds, and to [avoid] the manifest unfairness of continuing to shelter oneself behind other observers; albeit the shield of Professor Prestwich is used with great advantage." No one has had the experience possessed by Mr. Whitaker in the area he now describes, and all will be glad to welcome the expression of his opinions.

Special interest is usually concentrated on debateable subjects, and although certain differences of opinion are maintained on the classification of some of the Eocene divisions, there is little dispute about the position of the beds; the literature of London geology has increased most largely by the discussion of the Thames Valley deposits and their relation to the Glacial epoch. More recently the subject of underground geology has aroused a great deal of attention, for the deeper borings have proved the presence of rocks not previously suspected to occur beneath the area.

Special interest may therefore be said to be divided between the Underground Geology and the Pleistocene Geology.

The former is becoming more and more a practical subject. Speculations on the behaviour of rocks, especially of Palæozoic rocks beneath thick accumulations of Secondary strata, are necessarily hazardous. Were the whole of the West of England smothered up with Chalk and London Clay (the South Wales Coal-field being exposed), we might know nothing of the Bristol Coal-field, if several deep borings had proved simply Old Red Sandstone in some places and Silurian rocks in others. This up to the present time has been practically the result of deep borings under the London area.

Mr. Whitaker gives a full account of all that has been previously written on the plain of older rocks that lies under London. He is careful to point out that there is no evidence of a ridge beneath the area, though it would seem there may be something of the sort northwards, for the older rocks come nearer the surface at Ware than they do beneath London or Harwich.

On the subject of most commercial importance he concludes (p. 46) "that Coal-Measures are likely to occur somewhere along the line of the Thames Valley, or in neighbouring tracts; and that those Coal-Measures are likely to yield workable coal. It is rash to attempt to foretell the future; but it seems to me that *the day will come when coal will be worked in the South-east of England.*"

The new boring at Streatham has given further particulars of the presence of the Great Oolite Series beneath the London area, and a full account of this important boring is now for the first time published.

By the aid of the many borings, Mr. Whitaker is enabled to discuss the underground range of the Jurassic and Cretaceous rocks, and it is somewhat remarkable to learn that there is no certain record of Lower Greensand in the area, though it must underlie the Gault near Risborough. From the Gault there is a gradual passage through the Upper Greensand to the Chalk; and this is an interesting fact when we remember that chalky conditions commenced in Gault times at Hunstanton.

In the description of the Cretaceous beds, the author acknowledges help from Mr. Jukes-Browne, and some modifications are made in the classification of the zones and rock-beds of the Chalk. Thus the Chalk Rock is assigned an independent position between Middle and Upper Chalk; the zone of *Belemnitella plena* is separated from the Melbourn Rock and put with the Lower Chalk; while the lowest

zone of the Lower Chalk is termed the zone of *Ammonites varians*. Without the aid of these rock-beds but little progress could have been made in mapping the several divisions of the Chalk, for fossils in this, as in other formations, are not always to be found when most wanted.

The London Clay has not furnished much material for the believer in definite zones, for the fossils are alike through the formation, although some species are more abundant at certain horizons. Over large areas the London Clay appears to be practically barren of organic remains, indeed Mr. Whitaker mentions that he spent some days in examining new railway-cuttings in the London Clay in Essex, without meeting with any palæontological reward.

In dealing with the Eocene strata, Mr. Whitaker speaks of the practical importance of separating deposits that can be mapped distinctly even if there are no great palæontological distinctions in their fossils. This is quite right if we wish to interpret properly the rocky structure of a country, and show the relations of the strata to the form of the ground. The general remarks on the Lower London Tertiaries, and the full account given of the History and Literature of the subject, are for the most part new. Herein the author criticizes recent observations and views of Mr. Starkie Gardner, Mr. G. F. Harris, and others.

The Drifts that irregularly overlie the Bagshot Beds and older strata have given much trouble in determining their respective ages. But the process of minute correlation, which to some minds appears an essential basis for geological happiness, cannot always be carried out; and would lead to much unprofitable anxiety in the matter of Drifts. There may be no palæontological evidence, lithological characters may be of no avail, and, more serious still, there may be no stratigraphical evidence. Hence we can well understand the reason for a chapter on "Deposits of Doubtful Age." In some instances where outlying patches of gravel occur on London Clay, it is impossible to determine whether or not they are of Pliocene ("pre-Glacial") age, whether they are Glacial gravels newer or older than the Boulder-clay, or whether they may have been derived in comparatively recent times (during the denudation of the country) from any one of these accumulations, or from the pebble-beds of Bagshot age.

The broad general distinction in the gravels seems to be this. The pebble-beds of Eocene age are almost entirely made up of flint. The "pre-Glacial" (and possibly Pliocene) gravels are made up of flint and quartz pebbles. The Glacial gravels contain in addition many pebbles of quartzite, and sometimes derived Jurassic fossils: and they are rudely stratified and sometimes contorted. The River gravels contain all ingredients and are more distinctly stratified. But in Drift deposits there are many exceptions to every rule, and much of the Glacial gravel of Finchley is indistinguishable from the presumably older "Pebbly Gravel" of the Geological Survey.

The Clay-with-flints is also one of the accumulations of Doubtful Age, a residue left during many ages and forming now, by the slow decomposition of Chalk-with-flints by atmospheric actions, leaving

the flints and earthy matter, together with clayey and loamy material washed from Tertiary strata.

The author makes many references to the terms Glacial, post-Glacial and pre-Glacial. The two latter terms (as he says) are inadmissible into schemes of classification from their having no definite significance; but they are conveniently used when our knowledge is very limited that is to say, when we find a bed that is older than Boulder-clay, and which cannot be definitely proved to be Pliocene; or when we find a bed that is newer than Boulder-clay, and yet not definitely Recent. As Mr. Whitaker remarks (p. 328), "Beds which are truly post-Glacial in a southern district, may be of the same age as others which are clearly Glacial in a northern one;" and these remarks apply to the Pleistocene Thames Valley Deposits.

We are glad to find that the author supports the view of the fluviatile origin of the Thames Valley gravels and brick-earths. The organic remains indeed clearly support this contention. The difficulties raised have been based on the mode of occurrence of the deposits; but it must be borne in mind that the greater part of the gravel is simply re-deposited Glacial gravel, ready made, and not, perhaps, transported any great distances by the river. In the course of the accumulation of the river-drifts, the river has deepened its channel, leaving terraces in some places at different levels; but there is no marked regularity in these terraces as we trace the course of the Thames Valley deposits from Maidenhead to London.

Following the account of the River Drift, we have a chapter on Alluvium, etc. We should have preferred to divide these subjects into Pleistocene and Recent Alluvial Deposits. It is true that, in the area described, the Alluvium is distinguished from the "River Drifts" by its fineness of texture, but that is a local feature, for in the higher courses of the Thames and its tributaries we find a good deal of Recent gravel; and Alluvium itself seems entitled to be called "River Drift," as much as the brick-earth and gravel of Pleistocene age. Instructive sections are given of the Alluvium shown in making the Tilbury Docks and in other places.

Chapters devoted to Physical Geology and to Economic Geology, etc., complete the first volume, and we may call especial attention to the remarks on Springs, Wells, and Water-supply. It is impossible here to enumerate all the matters of interest discussed in the volume, suffice it to say that no subject seems to have been neglected. The work is illustrated by many sections and some pictorial views, and with figures of flint-implements. Lists of fossils are also given. A small coloured index-map would have been useful, but we ought not to make the slightest complaint when we have such a wealth of information in two volumes, bound in cloth, for a total cost of eleven shillings! That London geology excites a good deal of scientific interest apart from its practical bearings, is shown by the large sale of Mr. Whitaker's Guide to the Geology of London, of which (we understand) a fifth edition has just gone to press, and this is an excellent introduction to the subject treated so fully in the volumes before us.

III.—Cosmic Evolution, being Speculations on the Origin of our Environment. By E. A. Ridsdale, A.R.S.M. 12mo. pp. 130. (London, H. K. Lewis, 1889.)

NOWADAYS we are considerably ahead of the Hebrew cosmogonist in the means for speculating on the origin of our environment; but still there is a large margin of the apparently "unknowable" left for future philosophers to minimize if they can. The recently established doctrine of Evolution favours speculations in this quarter, especially in the minds of those who have received their scientific training since this doctrine has become a faith.

The Geological Evolutionist is fortunate in being confined within certain limits both as regards time and space. But it is far otherwise with the Cosmic Evolutionist, who finds his conceptions rendered hazy by an Eternity which had no beginning and can have no end, and by a Space which is equally without limits. No wonder that in such speculations "the imagination" at times "is forced to overstep the safe boundary of reason" (p. 103). This is candid on the part of our author, whose object evidently is to arrive at the truth, so far as that is attainable by the finite mind of man.

It is not for us to review the "General Aspect" of Cosmic Evolution any further than by confessing our faith in the grand and philosophic conception of La Place as to the physical history of the Solar system. The author's inference also seems a fair one, "that the evolution of present matter from the fire-vapours of the Solar system was analogous to the evolution of the fire-vapours from the universal primordial vapour of all Space."

But we must now leave off playing at "high jinks" in the starry firmament, and stick, as far as we can, to our own planet. The evolution of the primordial forms of matter was doubtless attended with a gradual loss of heat (changed perhaps into planetary motion, since Energy cannot be lost), increase of density and diminution of chemical activity. If this principle is true, there will seem to be an almost unbroken connection between Inorganic and Organic Evolution. Indeed, it might almost be said that "the chemical evolution proceeded till it finally induced an environment wherein favourable forms were matured into Life."

What this chemical evolution is supposed to have been we are told in the first chapter, which may be regarded as a sermon preached upon the text of increase of chemical stability, or, as he puts it, the "survival of the most inert." This process has been going on from the earliest geological ages, so that "things must have been more lively, chemically speaking, in the Archæan period than now!" Moreover, it is probable, he says, that the bodies we call elements are merely arrangements of matter to suit the present environment, and that under different conditions they might be broken up. Change, then, ceaseless change, was the order of things. But the rate was much more rapid in the early stages, whilst, through the gradual survival of the more inert forms, it became slower. It was when this state of physical calm was fairly matured that the forces

acting in the evolution of Living Forms were first able to come into play.

The Organic Aspect of Evolution.—The Earth being now fitted for it, a new and original force came into play; one different in its tendencies and unlike in its action to any of the previously-existing forces of Nature. "This force was LIFE. Ever since its first manifestation, while Inorganic Evolution has proceeded contemporaneously with it though in a milder and more subdued form, its power and scope of working has steadily increased."

Palæontology, as at present understood, certainly affords no record of that interesting period in the Earth's history, when the Organic was evolved out of the Inorganic. It is quite in accordance with the author's general views that such an evolution did take place, although he scarcely ventures to say so. He considers, likewise, that this growth of the Organic out of the Inorganic could only take place at one stage in the development of the Earth, and that, if such a thing were possible now, a serious blow would be struck at the doctrine of Evolution generally.

It is most probable that the origin of Life will remain amongst the things unknowable: all that Palæontology can do is to trace the evidence backwards as far as practicable. In this connection some might be disposed to disagree with the author as to the evidence afforded of the alleged approximation of the two branches of life—animal and vegetable. Excluding such doubtful forms as *Eozoon*, which few now regard as having any connection with *organic* structure, the earliest certain forms of Life in the Lower Cambrian exhibit a considerable amount of differentiation, rendering it probable that Life had become a factor in the Earth's history for a considerable period antecedent to this epoch. Hence it is by no means improbable that far more primitive forms existed which may have shown something of the approximation to which the author refers. But where are their remains now? Shall we seek them in the Monian, the Pebidian, or any other of the rival systems, which appear to occupy the ground between the Cambrian and the Archæan?

Although quite disposed to agree with the author in his general contention, there are some other statements of his in connection with Palæontology which seem open to criticism. He says (p. 48) that in early times, when the geological forces were more active, that stock had a tendency to survive whose members varied most. In a certain sense this may be so, but the history of the tetrabranchiate Cephalopoda presents us with an instance in the opposite direction. The steady-going *Nautilus*, though born long before his cousin the ever-changing *Ammonite*, alone survives. In fact, the author himself (p. 51) observes that a family succumbing either to slow alteration of the environment, or to inter-racial competition, always varies violently during the process. Indeed, the most sluggish genera, such as *Lingula*, but little affected, perhaps, by the environment, and still less given to inter-racial competition, have been much the same throughout all epochs. In such a case the "survival of the most inert" is applicable to certain forms of life. But this

does not violate the still higher law, "the survival of the fittest"; it only serves to show that, under certain circumstances, even in the Organic world, the most inert may be the fittest to survive.

Whilst admitting that no force or forces, exactly similar to Life, are known in Nature, Mr. Ridsdale observes, that one or two of them are not so "utterly unlike as to render it necessary to postulate a supernatural interference with the course of Nature to account for its occurrence." Certainly, a logical evolutionist need not summon such a *Deus ex machina* as Creation to account for any phenomenon, even the admittedly obscure one of Life. The author thinks it may be possible in the future to bring yet closer the analogies between the Organic and Inorganic worlds, though up to the present time nothing very conclusive has been pointed out. Perhaps the nearest parallel is that force which determines crystalline structure. A similar idea, it will be remembered, was brought forward in a recent address to the Geological Society. W. H. H.

REPORTS AND PROCEEDINGS.

Nov. 6, 1889.—W. T. Blanford, LL.D., F.R.S., President, in the Chair.—The following communications were read:—

1. "Contributions to our Knowledge of the Dinosaurs of the Wealden and the Sauropterygians of the Purbeck and Oxford Clay." By R. Lydekker, Esq., B.A., F.G.S.

The first section of this paper was devoted to the description of the remains of Iguanodonts from the Wadhurst Clay near Hastings collected by Mr. C. Dawson. They were considered to indicate two species, for which the names *Iguanodon hollingtoniensis* and *I. Fittoni* had been proposed in a preliminary notice.

In the second section an imperfect metatarsus of a species of *Megalosaurus* from the Hastings Wealden was described, and shown to indicate a species quite distinct from the one to which a metatarsus from the Wealden of Cuckfield belonged. Two cervical vertebræ of a Sauropterygian from the Purbeck of the Isle of Portland were next described, and referred to *Cimoliosaurus portlandicus*, Owen, sp.

The concluding section described an imperfect skeleton of a large Pliosaur from the Oxford Clay, in the collection of Mr. A. N. Leeds, which indicated a species intermediate between the typical Kimeridgian forms and the genus *Peloneustes*. These specimens were considered as probably referable to *Pliosaurus ferox*. Evidence was adduced to show that *Pliosaurus Evansi*, Seeley, should be transferred to *Peloneustes*.

2. "Notes on a 'Dumb Fault' or 'Wash-out' found in the Pleasley and Teversall Collieries, Derbyshire." By J. C. B. Hendy, Esq. Communicated by the President.

The "Top Hard" Seam of the district is being worked in these collieries at a depth of 500 yards, where it has an average thickness of five feet, with a band of cannel in the middle. In the working it was found that the coal began to thicken, until it became double the

usual size, the cannel also increasing in the "Top Seam," but in the Lower Seam running out altogether.

This double thickness of coal continued till the "Wash-out" was reached, when both coal and shaly roof disappeared, the space being replaced by sandstone similar to that of the beds overlying the shale. The clay floor of the Lower Seam had not been much interfered with, and this was followed for sixty yards, when the doubly thick seam was again met with, and on being followed gradually assumed its normal thickness.

No fossils have been noted in the "Wash-out" itself, the vertical extension of which is unknown.

3. "On some Palæozoic Ostracods from North America, Wales, and Ireland." By Prof. T. Rupert Jones, F.R.S., F.G.S.

The chief materials referred to were:—

1. Some good specimens of North-American Ostracoda from the Lower Helderberg and Cincinnati Groups in the British Museum, and the author's collection; these have given occasion for a critical revision and careful illustration of several forms.

2. In the 'Palæontology of New York,' vol. iii. 1859, several of the Palæozoic Ostracoda of New York State were described but not figured. Copies of some of the original drawings have been courteously supplied, with Dr. James Hall's permission, by Mr. J. M. Clarke, of Albany. They enlarge our knowledge of the Lower Helderberg fauna.

3. A large collection of Palæozoic Ostracoda, collected in the Lake Champlain district and elsewhere, sent by Prof. R. P. Whitfield, of New York, for examination by the author.

4. Other specimens belonging to the Utica Slate Series from Ontario, presented to the author by Dr John Young.

5. An interesting series of Lower Silurian (Ordovician species from near Welshpool, comprising a characteristic Cincinnati species, sent by Mr. J. Bickerton Morgan.

6. A rare Palæozoic Cytheroid Ostracod from Kildare, collected by Mr. Joseph Wright, F.G.S.

The specimens were described as nearly as possible in the order of their natural relationships and thus, besides adding to the known forms, they were shown to illustrate the modifications exhibited by the genera and species of these minute bivalved Crustaceans, both in limited districts and in different regions.

Amongst the forms described were the following new species and variety:—*Primitia mundula*, Jones, var. *cambrica*, nov.; *P. humilior*, sp. nov.; *P. Morgani*, sp. nov.; *P. Ulrichi*, sp. nov.; *P. Whitfieldi*, sp. nov.; *Entomis rhomboidea*, sp. nov.; *Strepula sigmoidalis*, sp. nov.; *Beyrichia Hallii*, sp. nov.; *Isochilina lineata*, sp. nov.; *I.? fabacea*, sp. nov.; *Leperditia Claypolei*, sp. nov.; *Xestoleberis Wrightii*, sp. nov.

The Rev. E. Tenison Woods.—We regret to record the death of this well-known Australian Geologist, which occurred at Sydney, New South Wales, on the 9th October, 1889.

INDEX.

ACT

ACTION of Frost on Soilcap, 255.
——— Pure Water on Mica, 187.
Address to the Devonshire Association, 500, 558.
——— Geological Section, 461.
Altered Igneous Rocks of Tintagel, 53, 101.
Amblypristis Cheops, from the Eocene of Egypt, 28.
America, North, Subaërial Deposits of, 289, 342.
Ammonites, Jurassic, 200.
Amphibians and Reptiles, Nomenclature of, 325.
Amygdaloids of the Tynemouth Dyke, 481.
Analysis of Fullers Earth, 455, 526.
——— Gault and Greensand, 456.
——— Kentish Rag, 73.
Annual General Meeting, Geological Society, 181.
—— Report, Geological Survey of Canada, 130.
Archæan Controversy, 319.
Arctic Ocean during the Mammoth Period, 305.
Ascoceras Murchisoni, Barrande, 121.
——— The Genus, 44.
Ashpringtón Volcanic Series, 332.
Attempt to Compute Geological Epochs, 277.
Australia, New Silurian *Protaster* from, 24.

BAGSHOT Beds and their Stratigraphy, 380.
Ballantrae Rocks of South Scotland, 20, 59.
Baron, Rev. R., Geology of Madagascar, 234.
Barrois, C., Fauna of the Erbray Limestone, 276.
——— Index to British Cretaceous Fossils, 35.
Bate, C. Spence, Obituary of, 526.
Bather, F. A., On a new Genus of Cronoidea from Bavaria, 87; Scientific Bibliography, 189; The Basals of Eugeniacrinidæ, 239.
Baur, G., on *Scaphognathus*, Newton, 171, 288.

CAN

Bavaria, Geological Survey of, 330.
Beecher, on the Brachiospongida, 232.
Bethesda, North Wales, Lower Cambrian at, 8.
Beyrichia Devonica, Jones, 386.
Blake's Geology of the Country around East Dereham, 87.
Blake, F. J., The Genus *Ascoceras*, 44; The Monian System, 45.
Bletchley, Granite at, 356.
Blytt, A., Displacement of Beach-lines, 277.
Bolton, H., Fish Remains from the Coal-Measures, 428.
Bonney, Prof. T. G., on Crystalline Rocks of the Alps, 40; The Serpentine of the Lizard, 44; Occurrence of a Variety of Picrite in Sark, 109; Dyke in the Lizard Serpentine, 189; Pebbles in the Cambrian of St. Davids, 315; Effects of Pressure on Crystalline Limestone, 483.
Breccia and Hornblende-Schist at Housel Cove, 114.
Bristow, H. W., Obituary of, 381.
Britain, Archæan Controversy in, 319.
British Association at Newcastle, 461.
——— Columbia, Glaciation of, 350.
Brontops robustus, Marsh, 99.
Brown, H. T., Permian Rocks of Leicestershire, 35.
Browneichthys ornatus, A. S. Woodward, 455.
Buckman, S. S., Uniformity of Scientific Bibliography, 94; On Jurassic Ammonites, 200; Cotteswold, Midford and Yeovil Sands, 185; The Genus *Acanthothyris* 329; Descent of *Sonninia*, and *Hammatoceras*, 370.
Bulletins, Geological Society of France, 329.

CALIAPORA, on the Genus, 432.
Callaway, Dr. C., Monian System, 94; Secondary Minerals in Crystalline Rocks, 285; The Archæan Controversy, 319; Foliation in the Malvern Hills, 335.
Cambrian of St. Davids, Pebbles in the, 315.
Canada, Discovery of *Turrilepas* in, 271.

CAN

Canada, Glaciation of, 211.
Carbonia fabulina, var. *altilis*, Jones and Kirkby, 270.
Carboniferous Gasteropoda, 380.
Carter, J., on Fossil Isopoda, 193.
Casquets and Rocks of Alderney, 331.
Catalogue of Fossil Cephalopoda, 393.
———— Fishes, 366.
———— of the British Isles, 81.
Cervus rectus, Newton, 145.
Chalmers, R., Glaciation of Eastern Canada, 211.
Champernowne, A., Ashprington Volcanic Series, 332.
Chandler, C., on Lacustrine Deposits, 376.
Chapman, F., Foraminifera of the London Clay, 498.
Chelonian Remains from the Wealden, 377.
Chemical and Physical Studies in the Metamorphism of Rocks, 567.
Chlorite Schists and Greenstones of South Devon, 265.
Circumpolar Lands, 305.
Clifford's Richmond Coal-Fields, Virginia, 138.
Clough's Geology of the Cheviot Hills, 86.
Clupea vectensis from the Isle of Wight, 40.
Coal-plants, British, 457.
Coal-seams, Modes of Formation of, 308.
———— of Western Australia, 240, 432.
Coccosteus compared with *Homosteus*, 1.
Cœlonautilus, Muscular Impressions of, 494.
Cœluroid Dinosaur from the Wealden, 119.
Cole and Jennings, on the Slopes of Cader Idris, 286.
Colloid Silica in the Chalk of Berks and Wilts, 237.
Comparison of European and American Dinosauria, 204.
Cosmic Evolution, 573.
Cope, E. D., on the Proboscidea, 438.
Creeping of the Soil-cap by Action of Frost, 255.
Crick, G. C., on Shell-muscles of *Cœlonautilus*, 494.
Crick, W. D., and Wilson, Lias Marlstone of Tilton, 296, 337.
Croll, J., Glacial Periods, 140; Rate of Subaerial Denudation, 526.
Crystalline Limestone, Pressure on, 483.
———— Rocks of the Alps, 40.

ECH

Cystechinus crassus from Barbadoes, 380.

DAMES, Dr., on *Amblypristis Cheops*, 28; The Ganoids of the Muschelkalk, 459.
Damon, R., Obituary of, 336.
Darent Valley, Discovery of Mammoth in the, 113.
Davison, C., Uniformity in Scientific Bibliography, 47; Secular Straining of the Earth, 220; Creeping of the Soil-cap by Action of Frost, 255; Stone-Rivers of the Falkland Islands, 390; on the Mean Rate of Subaërial Denudation, 409.
Dawson, G. M., Glaciation of British Columbia, 350.
———— J. W., Rocks of the Atlantic Coast of Canada, 236.
Deciduous Septa of *Ascoceras Murchisoni*, 121.
Deecke, W., Lias Fish-remains of Alsace, 428.
Deeley, R. M., on the Boulder Clay in Derby, 224.
De Gregorio, A., Note on *Pleurotoma turbida*, 78.
"Dendrodont" Fishes, 490.
Devonian Cephalopoda and Gasteropoda, 29.
———— Crustacea, on some, 28.
———— Fossils, 385.
———— Ganoid *Onychodus*, 499.
———— of South Devon and West Germany, 328.
Dinosauria of Europe and America, 204.
Dinosaurian Remains, 352, 575.
Dipterus macropterus, Traquair, 98.
———— New Species of, 98.
Division between the Lias and Oolite, 188.
Donald, Miss J., on Carboniferous Gasteropoda, 380.
Drainage of the English Lakes, 150.
"Dumb-Fault" or "Wash-out," 575.
Dyke in the Lizard Serpentine, 189.
Dykes and Beds, Local Thickening of, 69.

EARTH'S Crust, Some Physical Changes in the, 49, 115, 165.
Earth, Secular Straining of the, 220, 275.
Earthquakes, 521.
Echinocaris Whidbornei, Jones, 385.

ELV

Elvans and Volcanic Rocks of Dartmoor, 238.
Eocene and Mesozoic Chelonia, 141.
Eodiadema granulata, Wilson, 339.
Erbray Limestone, Fauna of the, 276.
Etheridge's Fossils of the British Isles, 81.
Etheridge and Willett, Dentition of *Lepidotus maximus*, 142.
Eugeniacrinidæ, the Basals of, 239.
Eurycormus grandis, A. S. Woodward, 449.
Exposure of Boulder-clay in Derby, 244.
"Eyes" of Pyrites, 396.

FACETTED Stones of the Salt Range, 415.
Falkland Islands, Stone Rivers of the, 390.
Felsites of the South-East of Ireland, 545.
Fisher, Rev. O., The Beds of the London Area, 48; On the Secular Straining of the Earth, 275.
Fishes, British Jurassic, 448.
Flint Implements in the Neighbourhood of Ightham, 142.
Foliation in the Malvern Hills, 335.
Foord, A. H., on *Ascoceras Murchisoni*, 121; Catalogue of the Fossil Cephalopoda, 363; Shell-muscles of Cœlonautilus, 494.
Formation of Coal-seams, 308.
Foraminifera, Bibliography of the, 34.
——·——— from the London-clay, 498.
Forsyth-Major, C., Discoveries in Samos, 431.
Fossil Fauna of Sweden, 124.
——— Isopods, 193.
Fossils from the Limestones of South Devon, 78.
Fouqué, E., on Earthquakes, 521.
France, Société géologique de, 238, 329.
Fulgurites from Monte Viso, 42.
Fullers Earth of Nutfield, 455, 526.

GARDNER, J. S., Mesozoic Monocotyledon, 144.
Gault and Greensand, Analysis of, 456.
Geikie, Dr. A., History of Volcanic Action, 32.
——— Prof. J., Address to the British Association, 461.
Geological Excursion to the Swiss Alps, 250.

HUD

Geological Society of London, 35, 87, 140, 181, 233, 285, 331, 376, 575.
——— Survey of Bavaria, 330.
——— ——— — Canada, 130, 517.
——— ——— — England and Wales, 86.
——— ——— — New South Wales, 276.
——— ——— — Ohio, 48.
Geology of Devon, 500, 558.
——— — London, 459, 569.
——— — Madagascar, 234.
Glacial Geology, 155.
——— Periods, 140.
Glaciation of British Columbia, 350.
——— — Eastern Canada, 211.
Goldfields of Western Australia, 240.
Goodchild, J. G., on the Formation of Coal-seams, 308.
Granite in a Boring at Bletchley, 356.
Graptolites from Dease River, 30.
Greenstone and Associated Rocks, 425.
Greenstones and Schists of South Devon, 265.
——— of Wicklow, 261.
Gregory, J. W., on a new Protaster from Australia, 24; on *Cystechinus crassus*, Gregory, 380.
Groom, T. T., A New Form of Tachylyte, 45.
Growth of Crystals in Igneous Rocks, 90.

HALL, Captain Marshall, Correspondence, 480.
Harker, A., The Physics of Metamorphism, 15; Local Thickening of Dykes, 69; "Eyes" of Pyrites, 396.
Hatch, Dr. F. H., on Soda Felsites in Wicklow, 70, 288; Petrographical Characters of Rocks from Madagascar, 235; Notes on the Wicklow Greenstones, 261; on Lower Silurian Felsites, 545.
Head of *Hybodus Delabechei*, 427.
Hendy, J. C. B., on Dumb Faults, 575.
Hicks, Dr. H., on *Stenotheca*, 288.
Hill, Rev. E., Rocks of Alderney, 331.
—— and Jukes-Browne, Colloid Silica, 237.
Histionotus angularis, Egerton, 241.
History of Tertiary Volcanic Action in Britain, 32.
Hof, Lower Palæozoic Rocks of, 411.
Homosteus compared with *Coccosteus*, 1.
Hornblende Schists of the Lizard, 332.
Howorth, H. H., on the Mammoth Period, 305.
Hudleston, W. H., The Geology of Devon, 500, 558.

Hughes, Prof. T. McKenny, The Lower Cambrian of Bethesda, 8, 96.
Hull, Dr. E., Terrestrial Magnetism, 326.
Human Relics found with Bones of Mastodon, 192.
Hutchings, W. M., Altered Igneous Rocks, 53, 101; on Ottrelite in North Cornwall, 214.
Hyland, J. S., Dr. on Soda-Microcline, 160; on Zonal Structure in Olivine, 492.
Hypsocormus Leedsi, A. S. Woodward, 450.
——— *tenuirostris*, A. S. Woodward, 451.

ICHTHYOSAURUS acutirostris, Zetlandicus, and *longifrons*, 44.
——— Paddle showing Integuments, 388.
Iguanodon Fittoni, Lydekker, 354.
——— *hollingtoniensis*, Lydekker, 355.
Index to Cretaceous Fossils of England and Ireland, 35.
India, Stones of the Salt Range of, 451.
Ireland, Silurian Felsites of the South-East of, 545.
Ireland, Royal Geological Society of, 187.
Irving, Rev. A., on Metamorphism of Rocks, 567.
Islands, Notes on the Ponza, 529.

JACK'S Mineral Wealth in Queensland, 226.
Jervis, on the Subterranean Treasures of Italy, 174.
Johnstone, A., Action of Water on Mica, 187.
Johnston-Lavis, Dr. H. J., on Sodalite Trachyte, 74; Notes on the Ponza Islands, 529.
Jones, T. R., Prof. A South African Geologists' Association, 144; Palæozoic Ostracoda, 576.
Jones and Kirkby, on the Ostracoda, 269.
—— and Woodward, New Devonian Fossils, 385.
Judd, Prof. J. W., Growth of Crystals in Igneous Rocks, 90; Tertiary Volcanoes of the Western Isles of Scotland, 91; Statical and Dynamical Metamorphism, 243.
Jukes-Browne, A. J., Granite at Bletchley, 356.
Jurassic Ammonites, 200.
——— Clays of Lincolnshire, 334.
Jurassic Fishes, 448.
——— Pisolite, 196.

KAYSER, E. von, Upper Devonian of Devonshire, 328.
Kentish Rag, Analysis of the, 73.
Keratophyres, Occurrence of, in Wicklow, 70, 288.
Kilimandscharo, Soda-Microcline from, 160.
Kilree, J. R., Movements in the Earth's Crust, 334.
Kirkby and Jones, on Ostracoda, 269.

LACUSTRINE Deposits in Suffolk, 376.
Lake District, Drainage of the, 150.
Lamplugh, G. W., Subdivisions of the Speeton Clay, 233.
Lapworth, Prof. C., The Ballantrae Rocks of South Scotland, 20, 59; Graptolites from Dease River, 30; *Olenellus* Zone in North-West Europe, 190.
Leedsichthys problematicus, A. S. Woodward, 457.
Lepidotus maximus, Dentition of, 142.
Lewis, Prof. H. C., Work on Glacial Geology, 155.
Lias in South-Eastern Scania, 123.
—— Marlstone of Tilton, 296, 337.
Lindström, Prof. G., List of Swedish Fossils, 124.
Line of Descent of the Invertebrata, 280.
List of Fossils from the Marlstone, Tilton, 341.
—— — Papers read before Section C, British Association, 478.
Lizard, Breccia and Hornblende Schists at the, 114.
——— Greenstone, 425.
Local Thickening of Dykes and Beds by Folding, 69.
London Area, Beds of the, 48.
——— Clay, Radiolaria of the, 39.
Lower Cambrian of Bethesda, 8.
Lydekker, R., Affinities of Five Genera of Mesozoic Reptiles, 39; on *Ichthyosaurus acutirostris*, 44; a Cœluroid Dinosaur, 119; Eocene and Mesozoic Chelonia, 141; a Wooden Dinosaur, 191; Nomenclature of Fossil Reptilia, 325; Wealden and Purbeck Chelonia, 377; *Ichthyosaurus* Paddle showing Integuments, 388; Secondary Reptilian Remains, 575.
Lyons, H. G., on the Bagshot Beds, 380.

MAG

MAGNETISM and the Earth's Crust, 486, 535.
Malton Museum, Palæontology in the, 361.
Mammoth Remains in the Darent Valley, 113.
Mansel-Pleydell, J. C., on *Histionotus angularis*, Egerton, 241.
Marine Deposits of the Indian Ocean, 514.
Marr, J. E., Drainage of the Lake District, 150; Palæozoic Rocks of Bavaria, 411.
Marsh, O. C., Restoration of *Brontops robustus*, 99; Comparison of European and American Dinosauria, 204.
Marshall Hall, Capt., Excursion to the Swiss Alps, 250.
Mathtews, G. F., on *Stenotheca*, 210.
McMahon, C. A., on Hornblende Schists, 332.
Mean Rate of Subaërial Denudation, 409.
Megalosaurus Oweni, Lyddeker, 325.
Mesozoic Monocotyledon, 144.
——— Strata of Sweden, 124.
——— Reptiles, 39.
Metamorphism, On the Physics of, 15, 96.
——— Statical and Dynamical, 243.
Microscopic Fauna of Cracow, 328.
——— Structure of Jurassic Pisolite, 196.
Morberg, J. C., Lias in South-Eastern Scania, 123.
Monian System, 45, 94.
Murray, Dr. J., Marine Deposits in the Indian Ocean, 514.

NAPLES, Sodalite Trachyte discovered in, 74.
Naumann, E., Terrestrial Magnetism, 486, 535.
Neumayr, M., Descent of the Invertebrata, 280.
New South Wales, Geological Survey of, 276.
—— Species of Fossil Isopod, 193.
Newton, E. T., on *Clupea vectensis*, Newton, 40; Vertebrate Fauna of Norfolk Forest Bed, 145; New Dinosaurian Remains, 352.
——— on *Pterosauria*, 171, 288.
Nicholson, Prof. H. A., on *Syringolites Roemeria*, 432.
Nodular Felstones from the Lleyn Peninsula, 186.
Nomenclature of Fossil Reptiles, 429.
Northampton, The Middle Lias of, 429.
Nova Scotia, Ostracoda from, 269.

PON

OBITUARY of C. Spence Bate, 526; H. W. Bristow, 381; R. Damon, 336; Rev. E. Tenison Woods, 576.
Occurrence of Radiolarians in Cretaceous Strata, 80.
——— — Soda-Felsites in Wicklow, 70, 288.
Olenellus Zone in North-West Europe, 190.
Olivine, Zonal Structure of, 492.
Onychodus in Spitzbergen, 499.
Origin of Movements in the Earth's Crust, 334.
Orinosaurus capensis, Lydekker, 353.
Ornithopsis, Note on the Pelvis of, 237.
Ostracoda from Nova Scotia, 269.
Ostracoda, Palæozoic, 576.
Ottrelite in North Cornwall, 214.

PADDLE of *Ichthyosaurus* showing Integuments, 388.
Palichthyology, Dr. K. von Zittel, on, 125, 177, 227.
Palæga McCoyi, Carter, 195.
Palæontological Discoveries in Samos, 431.
——— Record, A, 381.
Palæontology in the Malton Museum, 361.
Palæozoic Rocks of the Fichtelgebirge, 411.
Paros, Marble Quarries of the Isle of, 528.
Pavlow, A., Jurassic and Cretaceous Rocks of Russia, 520.
Pebbles in the Cambrian of St. Davids, 315.
Pebbly Sands of Suffolk, 377.
Pelorosaurus armatus, Lydekker, 325.
Permian Rocks of the Leicestershire Coal-field, 35.
Petrographical Characters of Rocks of Madagascar, 235.
Phillipsastræa, d'Orb., On, 398.
Physical Changes in the Earth's Crust, 49, 115, 165.
Physics of Metamorphism, 15, 96.
Physiography of the Lower Trias, 549.
Picrite, A Variety of, in Sark, 109.
Pinna Fittonensis, Wilson, 338.
Place in the Sequence of Ballantrae Rocks, 20, 59.
Pleistocene Boulder Clay in Derby, 244.
Pleurotoma turbida and *P. colon*, Note on, 78.
Polyzoa from the Inferior Oolite of Dorset, 239.
Ponza Islands, Notes on the, 529.

PRE

Preglacial Forest Bed, 145.
Pressure on Crystalline Limestone, 483.
Prestwich, Prof. J., Remains of Mammoth in the Darent Valley, 113; Palæolithic Implement at Ightham, 142; Pebbly Sands of Suffolk, 377.
Proboscidea, On the, 438.
Proposals Concerning a Magnetic Survey, 486.
Protaster brisingoides, Gregory, 24.
Pterosauria, E. T. Newton on the, 171.
Ptychacanthus and *Tristychius*, 27.

QUEENSLAND, Mineral Wealth in, 226.

RAISIN, Miss C. A., Some Nodular Felstones, 186; Greenstones and Schists of South Devon, 265.
Reade, T. Mellard, on the Physiography of the Lower Trias, 549.
Relation between *Syringolites* and *Roemeria*, 432.
Report of the Geological Survey of Ohio, 48.
Reptilian Remains, 575.
Restoration of *Brontops robustus*, Marsh, 99.
Review of the Mesozoic Formations of Sweden, 124.
Reynolds's Geological Atlas, 523; Map of the Environs of London, 525.
Rhinobatus bugesiacus, Note on, 393.
Richmond Coal-Fields, Virginia, 138.
Ricketts, Dr. C., Physical Changes in the Earth's Crust, 49, 115, 165.
Ridsdale, E. A., on Cosmic Evolution, 573.
Rocks of the Atlantic Coast of Canada, 236.
Roberts, T., on Jurassic Clays, 334.
Royal Geological Society of Ireland, 187.
Russell, I. C., Subaërial Deposits of North America, 289, 342.
Russia, Secondary Rocks of, 520.
Rüst, Dr., on Cretaceous Radiolarians, 80.
Rutley, F., on Fulgurites from Monte Viso, 42; Tachylyte from near Glasgow, 379.

SANFORD, P. G., Analysis of the Kentish Rag, 73; Analysis of Fullers Earth of Nutfield, 455, 526; Analysis of Gault and Greensand, 456.

SWI

Schäfer, Dr. R., on *Phillipsastræa*, 398.
——— and Woodward, Palæontological Record, 381.
Scientific Bibliography, 189.
——— ——— Uniformity in, 47, 93, 94.
Scyelite, Occurrence of, in Sark, 109.
Secondary Mineral in Crystalline Rocks, 285.
Secular Straining of the Earth, 220, 275.
Seeley, Prof. H. G., on the Pelvis of *Ornithopsis*, 237.
Selachian Fish from the Lithographic Stone, 393.
Serpentine of the Lizard, 44, 94.
Shell-muscles of *Cœlonautilus*, 494.
Sheppy, Foraminifera from, 498.
Sherborn, C. D., Bibliography of the Foraminifera, 34; Uniformity in Scientific Bibliography, 93; Foraminifera of the London Clay, 498.
Shrubsole, W. H., Radiolaria of the London Clay, 39.
Silurian Sponges, 232.
——— Protaster from Australia, 24.
Slopes of Cader Idris, 286.
Société géologique de France, 238.
Soda-Microcline from Kilimandscharo, 160.
Sodalite Trachyte discovered in Naples, 74.
Somervail, A., Serpentine of the Lizard, 96; on Breccia and Hornblende-Schists, 114; Greenstone of the Lizard, 425.
Sonninia and *Hammatoceras*, Descent of, 370.
South African Geologists' Association, 144.
Speeton Clay, Subdivisions of, 233.
Statical and Dynamical Metamorphism, 243.
Stenotheca, Second Note on, 210, 288.
Stone, G. H., on Stones from the Salt Range, 415.
Stone Rivers of the Falkland Islands, 390.
Streatham, On a Deep Boring at, 38.
Stur, D., on British Coal Plants, 457.
Subaërial Denudation, 409.
——— Deposits of North America, 289, 342.
Subterranean Treasures of Italy, 174.
Swan, R., Marble Quarries of Paros, 528.
Swanage, *Histionotus angularis* from, 241.
Swiss Alps, Geological Excursion to the, 250.

SYS

Systematic Position of the "Dendrodont" Fishes, 490.

TACHYLYTE associated with Gabbro, 43.

——— from Victoria Park, Glasgow, 379.

Teall, J. J. H., Amygdaloids of the Tynemouth Dyke, 481.

Tertiary Volcanoes of Scotland, 91.

Terrestrial Magnetism, 326, 535.

Thompson, B. M., Lias of Northampton, 429.

Tin and Coal in Western Australia, 432.

Tintagel, Altered Igneous Rocks of, 53, 101.

Traquair, Dr. R. H., on *Homosteus* compared with *Cocosteus*, 1; on *Tristychius* and *Ptychacanthus*, 27; a New Species of *Dipterus*, 98; on "Dendrodont" Fishes, 490.

Trias, Physiography of the Lower, 549.

Triassic Ganoids, 459.

Trigonocrinus, a New Genus of Crinoidea, 87.

Tristychius and *Ptychacanthus*, Notes on the Genera, 27.

Turrilepas Canadensis, H. Woodward, 274.

——— in the Utica Formation of Canada, 271.

Tynemouth Dyke, Amygdaloids of the, 481.

UPHAM, W., on the Work of Prof. H. C. Lewis, 153.

VERTEBRATE Fauna of the Norfolk Forest-Bed, 145.

ZON

WALFORD, E. A., Polyzoa from Inferior Oolite, 239.

Walker, F. J., on Oolitic Brachiopoda, 329.

Wealden, a Cœluroid Dinosaur from the, 119.

Wethered, E., on Jurassic Pisolite, 196.

Whidborne, Rev. G. F., Devonian Crustacea, 28; on Devonian Cephalopods, etc., 29; Fossils of the Limestones of South Devon, 78.

Whitaker, W., Deep Boring at Streatham, 38; Geology of London and part of the Thames Valley, 568.

Whiteaves, J. F., Canadian Palæontology, 517.

Wicklow Greenstones, 261.

Wilson and Crick, Lias Marlstone of Tilton, 296, 337.

Wisniowski on the White Jura of Cracow, 328.

Wooden Dinosaur, 191.

Woodward, A. S., Palæontology in the Malton Museum, 361; Catalogue of Fossil Fishes, 366; on *Rhinobatus bugesiacus*, 393; *Hybodus Delabechei*, 427; on British Jurassic Fishes, 448; on the Devonian Ganoid *Onychodus*, 499.

——— Henry, Discovery of *Turrilepas* in Canada, 271.

——— H. P., Coal and Tin in Western Australia, 432.

Woodwardian Museum Notes, 396.

Woods, Rev. E. Tenison, Death of, 576.

Worth, R. N., Volcanic Rocks of Dartmoor, 238.

YORKSHIRE Philosophical Society, 329.

ZITTEL, Dr. K. von, on Palichthyology, 125, 177, 227.

Zonal Structure of Olivine, 492.

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